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AGRICULTURAL RESEARCH INSTITUTE
PUSA

PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From April 20, 1899, to November 23, 1899.

VOL. LXV.

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PROCEEDINGS

OF

THE ROYAL SOCIETY.

Report of the Kew Observatory Committee for the Year ending December 31, 1898.

The operations of The Kew Observatory, in the Old Deer Park, Richmond, Surrey, are controlled by the Kew Observatory Committee, which is constituted as follows:—

Mr. F. Galton, *Chairman.*

Captain W. de W. Abney, C.B., R.E.
Prof. W. G. Adams.
Captain E. W. Creak, R.N.
Prof. G. C. Foster.
Prof. J. Perry.
The Earl of Rosse, K.P.
Prof. A. W. Rucker.
Dr. R. H. Scott.
Mr. W. N. Shaw.
Lieut.-General Sir R. Strachey, G.C.S.I.
Rear Admiral Sir W. J. L. Wharton, K.C.B.

The work at the Observatory may be considered under the following heads:—

I. Magnetic observations.
II. Meteorological observations.
III. Seismological observations.
IV. Experiments and Researches in connexion with any of the departments.
V. Verification of instruments.
VI. Rating of Watches and Marine Chronometers.
VII. Miscellaneous.

I. MAGNETIC OBSERVATIONS.

The Magnetographs have been in constant operation throughout the year, and the usual determinations of the Scale Values were made in January.

The ordinates of the various photographic curves representing Declination, Horizontal Force, and Vertical Force were then found to be as follows:—

Declinometer. 1 cm. = 0° 8′·7.
Bifilar, January 11th, 1898, for 1 cm. δH = 0·00051 C.G.S. unit.
Balance, January 12th, 1898, for 1 cm. δV = 0·00050 C.G.S. unit.

Owing to the gradual secular change of declination, the distance between the dots of light upon the cylinder of the magnetograph had become too small for satisfactory registration, and it was found necessary to alter the position of the zero line. From a similar cause it was also found necessary to re-adjust the balance of the vertical force magnetometer.

During the past year two magnetic storms, or periods of considerable disturbance of the needles, have been registered, the first on March 14–15, the second on September 9–10.

The extreme amplitude of the March disturbance was horizontal force, 0·0050 C.G.S. unit, vertical force, 0·0057 C.G.S. unit, and declination, 1° 26′. In eight minutes, from 10.40 to 10.48 P.M. on the 15th, the horizontal and vertical components exhibited falls of 0·002 and 0·003 C.G.S. unit respectively. The most rapid change of declination occurred some thirty minutes later. Speaking generally, the most salient features were the large falls in both the horizontal and vertical components and the movement of the declination needle nearly 1° east of its normal position.

The second storm occurred on September 9—10. The principal disturbance commenced somewhat gradually about noon on the 9th, but one of its most striking features was an exceptionally rapid fall occurring simultaneously at 3.5 P.M. in the horizontal and vertical forces and in the westerly declination. The fall was so rapid as to be shown somewhat indistinctly on the photographic traces, but it amounted to at least 15′ in the declination and 0·0023 C.G.S. unit in the horizontal force. The recovery from this fall was also rapid.

The declination needle, on the same day, between 5.15 P.M. and 8.8 P.M. receded 54′ to the east, then turned and in the course of the next thirty-two minutes moved 59′ to the west. The horizontal force attained its extreme maximum and minimum at 2.42 P.M. and

8.30 P.M. respectively, the range amounting to 0.0050 C.G.S. unit (or about 1/37 of the whole component). Between 7.30 and 8.30 P.M., this element fell 0.0036 C.G.S. unit. The vertical force reached its maximum about 6 P.M., and its minimum about 8.30 P.M., but as the trace unfortunately got off the sheet near the minimum, it can only be said that the range of vertical force exceeded 0.0036 C.G.S. unit.

Both storms were presumably associated with the aurora simultaneously seen in the British Isles. The March storm was the largest recorded since August, 1894.

The hourly means and diurnal inequalities of the magnetic elements for 1898, for the quiet days selected by the Astronomer Royal, will be found in Appendix I.

A correction has been applied for the diurnal variation of temperature, use being made of the records from a Richard thermograph as well as of the eye observations of a thermometer placed under the Vertical Force shade.

The mean values at the noons preceding and succeeding the selected quiet days are also given, but these of course are not employed in calculating the daily means or inequalities.

The following are the mean results for the entire year:—

Mean Westerly Declination	17° 1′.4.
Mean Horizontal Force............	0.18364 C.G.S. unit.
Mean Inclination	67° 17′.6
Mean Vertical Force	0.43885 C.G.S. unit.

Observations of Absolute Declination, Horizontal Intensity, and Inclination have been made weekly, as a rule.

A table of recent values of the magnetic elements at the Observatories whose publications are received at Kew will be found in Appendix IA to the present report.

In September Professor Luigi Palazzo of the Ufficio Centrale di Meteorologia, Rome, paid a visit to the Observatory for the purpose of comparing the Kew magnetic instruments and his own.

Dr. van Rijckevorsel also spent some time in the summer in making a further comparison between his magnetic instruments and those at Kew.

Mr. Hough, Fellow of St. John's College, Cambridge, who has recently been appointed chief assistant at the Royal Observatory, Cape of Good Hope, visited the Observatory from August 18 to September 1, in order to gain a knowledge of the method of observing with the Unifilar Magnetometer and Inclinometer.

At the request of Professor Moos, director of the Colaba Observatory, Bombay, copies of the horizontal force, the vertical force, and the declination curves for certain selected days during the years 1892, 1893, and 1897 have been made and forwarded to him.

Information on matters relating to various magnetic data has been supplied to Dr von Bezold, Professor Milne, and Mr. Gray.

The Observatory has been visited by Dr A Schmidt, of Gotha, Professor Eschenhagen, of Potsdam, and Professor Liznar, of Vienna, members of the International Conference on Terrestrial Magnetism, which was held at Bristol in September.

In spring the unifilar magnetometer and dip circle, previously lent to the Jackson-Harmsworth Polar Expedition, were put in order and lent to Mr P Baracchi, Acting Government Astronomer, Melbourne Observatory, for observational use in Australia and New Zealand, or in Antarctic exploration, as he might decide Later in the year an old dip circle was put in order at the cost of Sir George Newnes, and lent for the use of the Antarctic Expedition, under Mr. Borchgrevink. It was also agreed that if Mr P Baracchi should be willing to transfer to Mr Borchgrevink the unifilar magnetometer and dip circle referred to above, the Committee would raise no objection, provided Sir G Newnes should become responsible for the safe return of the instruments.

A course of magnetic instruction was given to the two magnetic observers of Mr Borchgrevink's expedition, Mr Colbeck and Mr. Bernacchi, the latter of whom had already practised the use of magnetic instruments at Melbourne Observatory

II. METEOROLOGICAL OBSERVATIONS.

The several self-recording instruments for the continuous registration of Atmospheric Pressure, Temperature of Air and Wet-bulb, Wind (direction and velocity), Bright Sunshine, and Rain, have been maintained in regular operation throughout the year, and the standard eye observations for the control of the automatic records duly registered.

The tabulations of the meteorological traces have been regularly made, and these, as well as copies of the eye observations, with notes of weather, cloud, and sunshine, have been transmitted, as usual, to the Meteorological Office.

With the sanction of the Meteorological Council, data have been supplied to the Council of the Royal Meteorological Society, the Institute of Mining Engineers, and the editor of 'Symons' Monthly Meteorological Magazine.'

Electrograph.—This instrument worked in a satisfactory manner till May, when the action markedly deteriorated. Tests of the battery showed that its E.M.F. had fallen off considerably; this was so far remedied by cleaning and recharging the top row of cells. At the same time a new silk suspension was fitted to the needle of the electrometer, and the instrument generally overhauled, and a new scale determination was carried out.

The records remained satisfactory until November, when the battery potential again began to fall off rapidly. Between November 24 and 27 the whole sixty cells were cleaned and recharged with a satisfactory result, and on the latter date one-third of the cells were removed to contract the scale, in order to record high winter values, as explained in last year's Report

On several occasions it had been noted that the electrometer needle had a tendency to "set" when the acid in the interior jar had been in use for some time This "setting" largely interfered with the freedom of the needle. It has, however, been considerably reduced, by substituting a single platinum wire connection for the double gridiron form hitherto employed.

In May another portable electrometer, No. 80, was purchased from White, of Glasgow; it is furnished with some additions to the usual pattern, which experience at the Observatory suggested as likely to prove beneficial in reducing induction effects. This electrometer has been used since, with the older instrument, White, No 53, in obtaining the scale value of the self-recording instruments, determinations being made on February 7, April 1, May 26, June 16, September 6, and November 28

Inspections.—In compliance with the request of the Meteorological Council, the following Observatories and Anemograph Stations have been visited and inspected —Stonyhurst, Yarmouth, North Shields, Alnwick Castle, Fort William, Glasgow, Aberdeen, and Deerness (Orkney), by Mr. Baker; and Radcliffe Observatory (Oxford), Holyhead, Fleetwood, Armagh, Dublin, Valencia, Falmouth, and St Mary's (Scilly Isles), by Mr. Constable

III. SEISMOLOGICAL OBSERVATIONS.

The seismograph referred to in last year's Report was delivered by Mr. R Munro in March. It is of Professor J. Milne's "unfelt tremor" pattern, the motion recorded being that of a horizontal pendulum or boom with a long period of vibration (fifteen to eighteen seconds from rest to rest). It is intended to measure the tilting of the ground along an east-west line, the boom itself lying due north and south.

At the suggestion of Professor Milne, who visited the Observatory, the site selected for at least a temporary trial is in the basement, inside the double-walled wooden room, originally designed for pendulum observations, and sometimes used as a warm chamber for chronometers. At first difficulties were encountered from wandering of the boom, which is still too liable to get off its pivot; but the record has been, on the whole, satisfactory for the latter half of the year. The following table gives particulars respecting the time of occurrence and amplitude in seconds of arc of the largest movements actually recorded:—

Date	Time (G.M.T.). h.	m.		Amplitude ″
June 29	7	19·8	P.M.	2·5
„	„	21·8	„	3·4
„	„	26·7	„	3·0
„	„	31·4	„	2·2
August 31	8	34·9	„	2·7
„	„	37·0	„	1·5
„	„	37·8	„	1·7
„	„	40·7	„	1·6
November 17...	1	44·3	„	0·5
„	„	46·4	„	0·6
„	„	58·6	„	0·6

The times deduced for the commencement of the above-mentioned earthquakes were 6 h. 47·6 m., 8 h. 4·5 m., and 1 h. 37 m. respectively.

Without special very careful experiments it would be difficult to say what is the probable error in fixing the precise times. Independent measurements of the photographic trace may agree to 0·1 or 0·2 of a minute, but there is room for a certain amount of doubt as to the proper values to attribute to the time marks on the sheet.

In the case of the times of commencement of a disturbance the uncertainty is greater, because the movement may be initially infinitesimal, and because a tiny movement arising from a different source (such movements being not uncommon) might intervene.

IV. EXPERIMENTAL WORK.

Fog and Mist.—The observations of a series of distant objects, referred to in previous Reports, have been continued. A note is taken of the most distant of the selected objects which is visible at each observation hour.

Atmospheric Electricity.—The comparisons of the potential, at the point where the jet from the water-dropper breaks up, and at a fixed station on the Observatory lawn, referred to in last year's Report, have been continued, and the observations have been taken twice every month.

During October some simultaneous observations were made with the two portable electrometers, the one situated on the pillar in the garden, the other at the same height on a tripod stand, at some distance in the park. It is hoped that time will be found to repeat the experiments on sufficiently numerous occasions to allow some conclusions to be drawn

Aneroid Barometers—The experiments referred to in the last three "Reports" were continued in the early part of the year. The results have been discussed by the Superintendent in a paper recently published in the Society's 'Transactions'

Platinum Thermometry—The experimental work carried out at the International Bureau of Weights and Measures at Sèvres by Dr J A Harker in co-operation with Dr Chappuis has only just terminated It has comprised a careful comparison of certain platinum thermometers belonging to the Observatory with a gas thermometer belonging to the Bureau, over the range −30° C. to +600° C

Dr Harker brought back the platinum thermometers, resistance box, &c, to the Observatory late in December, and is about to be engaged in preparing the results for publication In view of this and other special thermometric work in contemplation, the Committee have temporarily secured the services of Dr Harker in the capacity of special assistant to the Superintendent

Experiments have been continued at the Observatory itself on the fixity of zero, and the general behaviour of platinum thermometers, which have shown, amongst other things, the expediency of carefully checking the behaviour of the "leads"

Experimental work on the comparison of platinum and mercury thermometers has also been continued, and it is hoped that it will shortly be possible, utilising the results of Dr. Harker's work at Sèvres, to issue certificates to high range mercury thermometers embodying the results of direct comparison

Mercury Thermometry—The experiments on thermometers of different kinds of glass made by Messrs Powell and Sons, to which reference was made last year, have been continued. Further thermometers are being made by Messrs. Powell, of a pattern suggested by the Superintendent, with which it is hoped to experiment at higher temperatures.

V. VERIFICATION OF INSTRUMENTS.

The subjoined is a list of the instruments examined in the year 1898, with the corresponding results for 1897:—

	Number tested in the year ending December 31	
	1897.	1898.
Air-meters	5	1
Anemometers	3	11
Aneroids	77	169
Artificial horizons	17	9
Barometers, Marine	167	122
„ Standard	101	58
„ Station	30	55
Binoculars	661	374
Compasses	51	44
Deflectors	4	3
Hydrometers	292	463
Inclinometers	5	5
Photographic Lenses	10	13
Magnets	2	2
Navy Telescopes	707	681
Rain Gauges	27	12
Rain Measuring Glasses	31	10
Scales	—	2
Sextants	694	750
Sunshine Recorders	10	15
Theodolites	29	26
Thermometers, Avitreous, or Immisch's	5	10
„ Clinical	17,270	17,962
„ Deep sea	119	79
„ High Range	37	56
„ Hypsometric	30	38
„ Low Range	71	94
„ Meteorological	2,874	3.296
„ Solar radiation		2
„ Standard	117	66
Unifilars	4	6
Vertical Force Instruments	4	
Declinometers	3	—
Total	23,457	24,434

Duplicate copies of corrections have been supplied in 84 cases.

The number of instruments rejected in 1897 and 1898 on account of excessive error, or for other reasons, was as follows:—

	1897	1898
Thermometers, clinical	156	173
„ ordinary meteorological	38	92
Sextants	98	106
Telescopes	66	60
Binoculars	28	30
Various	56	26

Two Standard Thermometers have been constructed during the year.

There were at the end of the year in the Observatory, undergoing verification, 7 Barometers, 550 Thermometers, 50 Sextants, 20 Telescopes, 59 Binoculars, 2 Hydrometers, 2 Sunshine Recorders, 5 Rain Measures, and 2 Rain Gauges

VI RATING OF WATCHES AND CHRONOMETERS

The high standard of excellence to which attention has been drawn in previous Reports has been maintained Although the number of watches sent for trial this year is less than last year, yet the general average is as good, and 66 movements have obtained the highest possible form of certificate (the class A, especially good), which involves the attainment of 80 per cent of the total marks

The 483 watches received were entered for trial as below - -

For class A, 383; class B, 73, and 27 for the subsidiary trial. Of these 17 passed the subsidiary test, 116 failed from various causes to gain any certificate, 55 were awarded class B, and 295 class A

In Appendix III will be found a table giving the results of trial of the first 50 watches which gained the highest number of marks during the year. The highest place was taken by Mr S Yeomans, Coventry, with a keyless going-barrel, Karrusel lever-watch, No. 76,152, which obtained 89 2 marks out of a maximum of 100

Representations having been made to the Committee that some changes were desirable in the system of marks and dates on certificates, a circular was issued (as mentioned in last year's Report) to ascertain the general opinion of manufacturers and others interested in the matter, but the replies received showed no unanimity of opinion in favour of any one specified change, whilst a considerable number were quite satisfied with the existing conditions. Finally some small alterations were made, mainly in matters of detail.

The objection to the certificates that sustained most support—though even on this question opinions were fairly divided—was that the date suggested to the customer, in the case of any but the most recently tested watch, a line of criticism that would not naturally have presented itself. In consequence it was urged that the possession of a

Kew certificate was a very doubtful advantage to any watch remaining unsold for several years in a retailer's hands. The Committee could not see their way to alter the invariable practice of dating Kew certificates, but they agreed, in order to minimise the source of complaint, that a watch tested at the Observatory not less than three years previously, should be admitted to a fresh trial at half the usual fee

Marine Chronometers—During the year, 70 chronometers have been entered for the Kew A and B trials, of these 33 gained certificates, 21 failed, and there are 16 in hand

The new cold-air chamber, to which a preliminary reference was made in last year's Report, has been completed, and has proved very convenient

It consists of three separate divisions, each isolated from the others, and separated by a 3-inch space packed with flake charcoal, this same packing being continued on all sides of the divisions, the size over all being 6¾ ft by 6½ ft by 3 ft.

The centre chamber, 3 ft by 3 ft. by 2 ft, is fitted with sliding racks for the chronometers, and the division on either side is for the ice This is supplied in blocks, which rest on boards, and drain away into a trap and gulley. The chronometer chamber is furnished with trays to hold potassic chloride for drying purposes, and with maximum and minimum thermometers.

The doors are packed with flake charcoal, and are so arranged that the ice stores can be filled or emptied without any disturbance of the chronometer chamber

VII MISCELLANEOUS.

Paper—Prepared photographic paper has been supplied to the Observatories at Hong Kong, Mauritius, Oxford (Radcliffe), and Stonyhurst, and through the Meteorological Office to Aberdeen, Fort William, and Valencia

Anemograph and Sunshine Sheets have also been sent to Hong Kong and Mauritius.

Gas Thermometer—Sir Andrew Noble, K.C.B, having generously offered to present a gas thermometer to the Observatory, and to defray the cost of sending an assistant to Berlin to study the method of using a similar instrument at the Reichsanstalt, at Charlottenburg, the Committee gladly accepted the gift. The construction of the instrument has not yet been completed.

Pendulum Observations—In July Mr. F. Laurin and another officer of the Royal Austrian Navy swung half second pendulums in the sextant room on the spot where observations were made some years ago by von Sterneck.

Electric Tramways.—During the year a variety of schemes have been promoted for applying electric traction on the trolley system to tram lines in the neighbourhood of the Observatory, and one of these schemes, promoted by the London United Tramway Company, for a new line between Kew Bridge and Hounslow, passing within 1,300 yards of the Observatory, has received the sanction of Parliament. The Committee, roused by the fate that has befallen the magnetic observatories at Toronto and Washington, requested Professor Rucker and Professor Perry to take the matter in hand. A series of experiments made at various places in London and Leeds, under the general supervision of Professor Rucker, showed that electric railways and tramways, satisfying presumably all the existing requirements of the Board of Trade, produced very sensible disturbances in a declinometer at distances of two or three miles. This fact was brought before the notice of the Royal Society, who in turn entered into communication with the Board of Trade, with the result that the following clauses were inserted in the London United Tramway Company's Bill:—

1. The whole circuit used for the carrying of the current to and from the carriages in use on the railway shall consist of conductors, which are insulated along the whole of their length to the satisfaction in all respects of the Commissioners of Her Majesty's Works and Public Buildings (in this section called the "Commissioners"), and the said insulated conductors which convey the current to or from any of such carriages shall not at any place be separated from each other by a distance exceeding one-hundredth part of the distance of either of the conductors at that place from Kew Observatory.

2. If, in the opinion of the Commissioners, there are at any time reasonable grounds for assuming that, by reason of the insulation or conductivity having ceased to be satisfactory, a sensible magnetic field has been produced at the Observatory, the Commissioners shall have the right of testing the insulation and conductivity upon giving notice to the Company, who shall afford all necessary facilities to the engineer or officers of the Commissioners, or other person appointed by them for the purpose, and the Company shall forthwith take all such steps, as shall in the opinion of the Commissioners be required for preventing the production of such field.

3. The Company shall furnish to the Commissioners all necessary particulars of the method of insulation proposed to be adopted, and of the distances between the conductors which carry the current to and from the carriages.

It is understood that the above clauses will be insisted on by the Board of Trade in the case of any other tram line which can be shown to be a probable source of danger to the Observatory.

The Committee are much indebted to Professor Rücker and Professor Perry for the trouble they have taken in the matter, and they are also glad to express their acknowledgment of the valuable assistance rendered by the editors of scientific journals and various eminent men of science in educating public opinion. The Committee even hope that ere long tramway companies themselves will recognise the benefits accruing from improved insulation

Whilst everything has been done, as far as can be foreseen, to protect the magnetographs, it is impossible to contemplate the future without some misgivings.

National Physical Laboratory — The Government Committee, referred to in last year's Report, visited the Observatory on January 18th. In the course of the summer, that Committee submitted to the Lords Commissioners of Her Majesty's Treasury a report, embodying the following four recommendations :—

1. That a public institution should be founded for standardizing and verifying instruments, for testing materials, and for the determination of physical constants

2. That the institution should be established by extending the Kew Observatory in the Old Deer Park, Richmond, and that the scheme should include the improvement of the existing buildings, and the erection of new buildings at some distance from the present Observatory.

3. That the Royal Society should be invited to control the proposed institution, and to nominate a Governing Body, on which commercial interests should be represented, the choice of the members of such Body not being confined to Fellows of the Society

4 That the Permanent Secretary of the Board of Trade should be an *ex officio* member of the Governing Body; and that such Body should be consulted by the Standards Office and the Electrical Standardizing Department of the Board of Trade upon difficult questions that may arise from time to time or as to proposed modifications or developments

In October, the Royal Society informed the Kew Observatory Committee that the Government had adopted the report generally, and were willing to provide funds for carrying it into effect; consequently the Royal Society asked for the concurrence of the Kew Observatory Committee in their action.

In reply, the Committee expressed their willingness to facilitate the execution of the scheme, and to continue to administer the Observatory pending the nomination of the new Governing Body. The arrangements were not completed before the close of 1898.

Library.—During the year the library has received publications from

20 Scientific Societies and Institutions of Great Britain and Ireland,

93 Foreign and Colonial Scientific Establishments, as well as from several private individuals.

The card catalogue has been proceeded with.

Audit, &c.—The accounts for 1898 have been audited by Mr W. B. Keen, Chartered Accountant, on behalf of the Royal Society, and by Professor Carey Foster on behalf of the Committee.

The balance sheet, with a comparison of the expenditure for the two years, 1897 and 1898, is appended.

PERSONAL ESTABLISHMENT.

The staff employed is as follows:—

C. Chree, Sc.D., F.R.S., Superintendent.
T. W. Baker, Chief Assistant.
E. G. Constable, Observations and Rating.
W. Hugo, Verification Department.
J. Foster ,, ,,
T. Gunter ,, ,,
W. J. Boxall ,, ,,
G. E. Bailey, Accounts and Library.
E. Boxall, Observations and Rating.
G. Badderly, Verification Department, and six other Assistants.
A Caretaker and a Housekeeper are also employed.

(Signed) FRANCIS GALTON,
Chairman.

Kew Observatory Account of Receipts and Payments for the year ending December 31st, 1898.

RECEIPTS

	£	s	d	£	s	d
To Balance from Year 1897				456	18	1
Royal Society —						
Gassiot Trust. Annual payment	443	11	2			
" " Income Tax returned	15	0	2			
				458	11	4
Meteorological Council —						
Allowance	400	0	0			
Postages, &c.	2	9	2			
				402	9	2
Tests —						
Verification	1575	16	0			
Rating	614	8	0			
Lenses	5	13	9			
				2195	17	9
Researches —						
Grant from Gunning Fund for comparisons of thermometer scales				120	0	0
Commissions executed for Colonial and Foreign Institutions, &c				500	0	0
Rents				7	[illegible]	0
Dividends on India Stock				43	19	6
Messrs D and J Welby for photographic residues				1	2	8
				£4426	1	8

PAYMENTS.

	£	s	d.	£	s	d
By Normal Observatory —						
Salaries—Observations, Tabulations, &c	536	15	6			
Incidental Expenses, Photographic Paper, &c	41	1	7			
Proportion of Administration Expenditure	187	10	0			
				765	7	1
Researches —						
Salaries	158	8	0			
Incidental Expenses, &c	64	9	2			
Proportion of Administration Expenditure	375	0	0			
				597	17	2
Tests —						
Salaries	918	6	0			
Incidental Expenses Apparatus &c	222	9	5			
Proportion of Administration Expenditure	499	4	7			
				1640	0	0
Commissions —						
Purchase of Instruments and Photographic Paper for Colonial and Foreign Institutions, &c	529	3	1			
Proportion of Administration Expenditure	187	10	0			
				716	13	1
Seismograph—Cost of apparatus and sundries				55	15	0
Balance—London and County Bank	633	4	3			
Awaiting Banking	2	18	8			
In hand (Petty Cash)	14	6	5			
				650	9	4
				£4426	1	8

ADMINISTRATION EXPENDITURE.

Particulars	£	s	d	*Apportionment*	£	s	d.
Superintendent	500	0	0	Observatory	187	10	0
First Assistant Librarian, &c	454	18	0	Researches	375	0	0
Rent, Fuel, &c	87	16	6	Tests	499	4	7
Caretaker, Repairs, &c	206	10	1	Commissions	187	10	0
	£1249	4	7		£1249	4	7

Audited on behalf of the Royal Society and found correct,
17th January, 1899
(Signed) W B KEEN, *Chartered Accountant*

Examined on behalf of the Kew Observatory Committee, and approved,
18th January, 1899
(Signed) G CAREY FOSTER.

ESTIMATED ASSETS.

	£	s.	d.	£	s.	d.
By Balance as per Statement				650	9	4
£1300 India 3½ per cent. Stock, value on January 1 1899				1515	5	0
Payments due —						
Meteorological Council—Allowance, Postages, &c	100	14	3			
Test Fees	610	2	9			
Commissions, &c	105	16	0			
				815	13	0
Stock:—						
Blank Forms and Certificates	57	10	4			
Standard Thermometers	76	12	0			
				134	2	4
				£3115	9	8

January 2[illegible], 1899.

ESTIMATED LIABILITIES.

	£	s.	d.
To Administration accounts—Gas, Rent, Repairs, &c	44	17	9
Observatory accounts—Photographic Paper, &c	14	14	4
Tests accounts—Repairs, Apparatus, &c.	14	19	0
Commissions	19	11	6
Researches	5	2	7
Grant from Gunning Fund for comparisons of thermometer scales	120	0	0
Unspent balance of Grant for Seismograph	4	5	0
General Balance	2891	19	6
	£3115	9	8

(Signed) CHARLES CHREE,
Superintendent.

Comparison of Expenditure during the Years 1897 and 1898.

Expenditure.	1897			1898			Increase.			Decrease		
	£	*s*	*d*	£	*s*	*d*	£	*s.*	*d*	£	*s*	*d*
Administration —												
Superintendent.	500	0	0	500	0	0						
First Assistant.. ...	331	18	0	333	8	0	1	10	0			
Office....	119	6	1	121	10	0	2	3	11			
Rent, Fuel, Lighting, &c	88	9	2	87	16	6				0	12	8
Caretaker....... ...	70	4	6	68	18	0				1	6	6
Incidental Expenses ..	113	2	3	137	12	1	24	9	10			
	1223	0	0	1249	4	7	28	3	9	1	19	2
Normal Observatory —												
Salaries—Observations, &c	320	2	10	336	15	6	16	12	8			
Incidental Expenses	48	1	4	41	1	7				6	19	9
Prop Adm Expenditure	244	12	0	187	10	0				57	2	0
Researches —												
Salaries.....	110	0	0	158	8	0	48	8	0			
Purchase of Apparatus, &c..........	209	11	1	64	9	2				145	1	11
Prop Adm Expenditure	366	18	0	375	0	0	8	2	0			
Tests —												
Salaries..............	898	11	6	918	6	0	19	14	6			
Incidental Expenses...	203	0	6	222	9	5	19	8	11			
Prop. Adm Expenditure	489	4	0	499	4	7	10	0	7			
Commissions —												
Purchases for Colonial Institutions, &c	398	18	2	529	3	1	130	4	11			
Prop Adm Expenditure	122	6	0	187	10	0	65	4	0			
Seismograph				55	15	0	55	15	0			
Gross Expenditure.... (showing an increase of £164 6*s* 11*d*).	3411	5	5	3575	12	4	373	10	7	209	3	8
Extraordinary Expenditure.												
Researches —												
Salaries...	110	0	0	158	8	0	48	8	0			
Purchase of Apparatus, &c	206	0	7	61	15	10				144	4	9
Commissions.—												
Purchases for Colonial Institutions, &c	398	18	2	529	3	1	130	4	11			
Seismograph.......... .				55	15	0	55	15	0			
	714	18	9	805	1	11	234	7	11	144	4	9
Leaving for Ordinary Nett Expenditure........... (showing an increase of £74 3*s*. 9*d*).	2696	6	8	2770	10	5	139	2	8	64	18	11

List of Instruments, Apparatus, &c., the Property of the Kew Observatory Committee, at the present date out of the custody of the Superintendent, on Loan

To whom lent.	Articles.	Date of loan
G. J. Symons, F.R.S.	Portable Transit Instrument................	1869
The Science and Art Department, South Kensington.	Articles specified in the list in the Annual Report for 1893.................	1876
Professor W. Grylls Adams, F.R.S.	Unifilar Magnetometer, by Jones, No. 101, complete.............	1883
	Pair 9-inch Dip Needles with Bar Magnets ...	1887
Lord Rayleigh, F.R.S.	Standard Barometer (Adie, No. 655)	1885
Radcliffe Observatory, Oxford.	Black Bulb Thermometer *in vacuo*	1897
Mr. P. Baracchi (Melbourne Observatory).	Unifilar Magnetometer, by Jones, marked N.A.B.C., complete...........	1898
	Dip Circle, by Barrow, with one pair of Needles and Bar Magnets	1898
	Tripod Stand	1898
The Borchgrevink-Newnes Antarctic Expedition.	Dip Circle, by Barrow, No. 24, with four Needles and Bar Magnets................	1898

APPENDIX I.

MAGNETICAL OBSERVATIONS, 1898.

Made at the Kew Observatory, Old Deer Park, Richmond, Lat. 51° 28′ 6″ N. and Long. $0^{h}\ 1^{m}\ 15^{s}{\cdot}1$ W.

The results given in the following tables are deduced from the magnetograph curves which have been standardised by observations of deflection and vibration. These were made with the Collimator Magnet K C I and the Declinometer Magnet marked K.O. 90 in the 9-inch Unifilar Magnetometer by Jones

The Inclination was observed with the Inclinometer by Barrow, No. 33, and needles 1 and 2, which are 3½ inches in length

The Declination and Force values given in Tables I to VIII are prepared in accordance with the suggestions made in the fifth report of the Committee of the British Association on comparing and reducing Magnetic Observations.

The following is a list of the days during the year 1898 which were selected by the Astronomer Royal, as suitable for the determination of the magnetic diurnal inequalities, and which have been employed in the preparation of the magnetic tables —

January	3,	4,	7,	9,	23
February	1,	3,	7,	26,	27.
March	1,	3,	4,	24,	31.
April....	1,	9,	21,	22,	29.
May	7,	19,	21,	23,	25
June..............	5,	13,	17,	20,	21.
July	2,	10,	15,	16,	18
August............	1,	8,	10,	15,	25.
September	6,	7,	12,	21,	26
October............	4,	8,	12,	16,	18
November	5,	10,	14,	29,	30.
December..........	11,	12,	17,	23,	26

Table I.—Hourly Means of the Declination, as determined from the

Hours	Preceding noon.	Mid	1.	2	3.	4.	5.	6	7	8.	9	10.	11.
	(17° +) West						Winter.						
1898 Months	′	′	′	′	′	′	′	′	′	′	′	′	′
Jan ..	6·1	3·3	3·5	3·8	3·9	3·6	3·4	3·2	3·0	2·9	3·0	3·3	4·8
Feb ..	6·0	3·0	3·2	3·3	3·3	3·4	3·3	2·9	2·8	2·7	2·4	3·1	4·7
March.	5·4	1·3	1·3	1·3	1·2	0·9	1·0	1·0	0·8	0·1	−0·4	0·5	2·9
Oct. ..	4·8	−1·7	−1·6	−1·5	−1·5	−1·5	−1·7	−1·7	−2·5	−3·3	−3·0	−0·8	1·8
Nov. ..	2·2	−1·6	−1·7	−1·1	−0·8	−0·9	−1·0	−1·0	−0·9	−0·9	−0·8	0·3	1·5
Dec. ..	1·8	−1·5	−1·3	−0·8	−0·7	−0·8	−0·7	−0·8	−1·1	−1·3	−1·1	−0·3	0·1
Means	4·4	0·5	0·6	0·8	0·9	0·8	0·7	0·6	0·4	0·0	0·0	1·0	2·6
							Summer.						
	′	′	′	′	′	′	′	′	′	′	′	′	′
April ..	6·2	0·6	0·8	0·6	0·5	0·4	0·1	0·3	−0·5	−1·0	−1·1	0·5	2·8
May ..	6·7	1·5	1·5	1·2	0·9	0·2	−0·8	−2·3	−3·4	−3·1	−2·1	1·4	4·8
June ..	5·7	1·1	1·0	0·9	1·0	−0·3	−1·8	−2·8	−3·0	−2·8	−2·4	−0·3	2·9
July ..	5·3	0·9	0·3	0·2	−0·3	−1·0	−2·3	−2·9	−2·9	−2·6	−1·6	0·5	2·7
Aug. ..	6·6	0·0	0·0	−0·3	−0·7	−1·0	−1·7	−1·9	−2·4	−2·4	−0·9	1·3	3·4
Sept. ..	6·4	−0·3	−0·5	−0·9	−0·8	−1·4	−1·8	−2·3	−2·4	−2·3	−1·6	0·8	3·2
Means	6·2	0·6	0·5	0·3	0·1	−0·5	−1·4	−2·0	−2·4	−2·4	−1·6	0·7	3·3

Table II.—Diurnal Inequality of the

Hours	Mid.	1.	2.	3	4	5.	6	7	8.	9.	10.	11.
						Summer Means						
	′	′	′	′	′	′	′	′	′	′	′	′
	−0·7	−0·8	−1·0	−1·2	−1·8	−2·7	−3·3	−3·7	−3·7	−2·9	−0·6	+2·0
						Winter Means						
	′	′	′	′	′	′	′	′	′	′	′	′
	−1·0	−0·9	−0·6	−0·6	−0·7	−0·7	−0·9	−1·1	−1·4	−1·4	−0·4	+1·2
						Annual Means.						
	′	′	′	′	′	′	′	′	′	′	′	′
	−0·8	−0·8	−0·8	−0·9	−1·3	−1·7	−2·1	−2·4	−2·6	−2·2	−0·5	+1·6

NOTE.—When the sign is + the magnet

selected quiet Days in 1898. (The Mean for the Year = 17° 1′·4 West.)

Noon.	1.		3.	4.						10	11.	Mid	Succeeding noon.
						Winter							
5·6	5·7	5·0	4·4	4·2	4·0	3·7	3·4	3·1	2·8	2·9	3·0	3·3	5·6
6·0	6·4	6·4	5·9	4·9	4·5	4·0	3·6	3·6	3·2	3·0	2·9	2·6	5·6
5·5	6·6	6·6	5·5	4·8	3·5	3·3	2·9	2·6	2·4	1·8	1·8	1·6	5·1
3·3	3·7	3·1	1·9	0·3	0·1	−0·1	−0·3	−0·8	−1·3	−1·6	−1·5	−1·8	4·5
2·6	2·9	2·0	1·2	0·8	0·6	−0·1	−0·4	−0·5	−0·8	−1·1	−1·1	−1·1	2·3
1·3	1·3	0·9	0·2	-0·1	-0·6	−0·6	−0·9	−1·1	−1·4	−1·3	−1·2	−1·3	1·1
4·1	4·4	4·0	3·2	2·1	2·0	1·7	1·4	1·2	0·8	0·6	0·7	0·6	4·0
						Summer.							
5·6	7·3	7·3	5·8	4·5	3·4	2·4	1·8	1·9	1·8	1·4	1·1	0·8	6·6
7·7	8·4	7·8	5·8	3·8	2·0	1·2	1·3	1·7	1·7	1·6	1·4	1·1	6·1
5·3	5·8	5·3	4·1	3·2	2·3	1·9	1·3	0·8	1·0	1·5	1·3	1·0	6·1
5·4	6·5	5·5	4·6	3·1	2·0	1·7	1·6	1·5	1·5	1·3	1·0	0·9	6·2
5·8	7·2	6·8	5·9	4·0	2·4	1·6	1·2	1·0	1·0	0·9	0·6	0·4	6·9
5·5	6·4	5·3	3·2	1·3	0·2	−0·1	0·3	0·1	-0·7	-0·5	−0·1	−0·6	5·1
5·9	6·9	6·3	4·9	3·3	2·1	1·5	1·3	1·2	1·1	1·0	0·8	0·6	6·2

Declination as deduced from Table I.

Noon	1.	2.	3	4	5	6	7.	8.	9.	10.	11.	Mid.
						Summer Means						
′ +4·6	′ +5·6	′ +5·0	′ +3·6	′ +2·0	′ +0·7	′ +0·1	′ −0·1	′ −0·1	′ −0·3	′ −0·3	′ −0·5	′ −0·7
						Winter Means.						
′ +2·6	′ +3·0	′ +2·5	′ +1·7	′ +0·9	′ +0·6	′ +0·2	′ −0·1	′ −0·3	′ −0·7	′ −0·8	′ −0·8	′ −0·9
						Annual Means						
′ +3·6	′ +4·3	′ +3·8	′ +2·7	′ +1·5	′ +0·7	′ +0·2	′ −0·1	′ −0·2	′ −0·5	′ −0·6	′ −0·6	′ −0·8

points to the west of its mean position.

Table III.—Hourly Means of the Horizontal Force in C.G.S. units (corrected

(The Mean for the

Hours	Preceding noon	Mid	1	2	3	4.	5	6.	7	8	9	10.	11.
	0·18000 +						Winter.						
1898 Months.													
Jan. . .	349	351	352	351	352	353	355	358	357	355	351	347	348
Feb. . . .	353	362	361	361	361	363	364	366	366	365	362	358	357
March .	346	356	354	356	357	355	357	359	360	358	351	345	340
Oct. . . .	355	369	370	369	366	368	369	368	366	361	353	348	348
Nov. . .	360	369	369	368	370	371	374	377	376	372	365	359	361
Dec . .	378	381	382	382	383	384	384	384	385	383	384	385	382
Means. .	358	365	365	364	365	366	367	369	368	366	361	357	356
							Summer						
April. . .	343	360	358	358	357	356	356	354	354	348	343	338	338
May . . .	362	373	372	369	369	369	367	362	352	345	342	340	341
June . . .	359	373	372	371	371	370	369	365	361	353	350	348	351
July . .	302	370	369	370	370	370	370	364	357	351	347	347	356
Aug . . .	358	378	375	373	373	372	369	366	362	356	351	349	355
Sept. . . .	333	355	356	357	354	352	351	348	344	339	334	328	331
Means. .	353	368	367	366	366	365	364	360	355	349	345	342	345

Table IV.—Diurnal Inequality of the

Hours	Mid	1	2.	3.	4.	5	6.	7.	8.	9.	10.	11.
						Summer Means						
	+ ·00005	+ ·00004	+ 00004	+ 00003	+ ·00002	+ 00001	− 00003	− 00008	− 00014	− 00018	− ·00021	− ·00017
						Winter Means						
	·00000	00000	− 00001	00000	+ ·00001	+ ·00002	+ ·00004	+ ·00003	+ ·00001	− ·00004	− ·00008	− ·00009
						Annual Means						
	+ ·00003	+ 00002	+ 00001	+ ·00001	+ 00001	+ 00002	·00000	− ·00002	− ·00007	− 00011	− 00015	− ·00013

Note.—When the sign is + the

for Temperature) as determined from the selected quiet Days in 1898.
Year = 0 18364.)

Noon	1	2	3.	4	5	6	7.	8	9.	10.	11.	Mid	Succeeding noon.
							Winter						
350	354	353	353	351	354	354	354	354	354	354	354	354	354
357	359	362	362	339	361	361	361	362	363	363	363	363	358
342	347	351	353	356	356	357	359	360	360	361	361	363	345
354	359	366	368	369	371	371	373	374	374	373	372	371	353
366	370	371	372	372	375	376	376	376	375	373	371	372	368
383	384	384	383	385	386	387	387	385	384	384	384	383	386
359	362	364	365	365	367	368	368	368	368	368	367	368	361
							Summer.						
343	350	353	354	356	360	366	366	365	363	360	361	361	342
347	353	360	364	369	374	376	380	381	380	378	377	375	361
359	365	371	371	373	376	378	380	379	376	375	373	372	355
363	365	369	375	375	377	379	380	378	380	379	376	375	360
361	361	363	366	370	374	380	382	383	384	382	382	380	364
340	349	351	352	354	357	360	363	365	365	362	362	362	350
352	357	361	364	366	370	373	375	375	375	373	372	371	355

Horizontal Force as deduced from Table III.

Noon	1.	2.	3	4	5	6	7.	8.	9.	10.	11.	Mid.
						Summer Means						
− ·00011	− ·00006	− ·00002	+ ·00001	+ ·00004	+ ·00007	+ ·00010	+ ·00013	+ ·00012	+ ·00012	+ 00010	+ 00009	+ ·00008
						Winter Means						
− ·00006	− 00003	− ·00001	·00000	·00000	+ ·00002	+ ·00003	+ 00003	+ ·00003	+ 00003	+ ·00003	+ ·00002	+ ·00003
						Annual Means						
− 00008	− ·00004	− ·00001	+ ·00001	+ ·00002	+ ·00005	+ ·00006	+ ·00008	+ ·00008	+ ·00008	+ ·00006	+ ·00006	+ ·00005

reading is above the mean.

Table V.—Hourly Means of the Vertical Force in C.G.S. units (corrected
(The Mean for the

Hours	Preceding noon.	Mid.	1	2	3	4.	5	6	7.	8	9	10	11.
	0·43000 +					Winter							
1898 Months													
Jan ...	892	899	899	899	899	899	898	897	897	896	895	895	897
Feb ...	897	902	902	901	901	901	901	901	900	900	900	898	896
March ..	891	908	908	908	906	905	905	904	901	904	902	897	891
Oct ...	850	862	862	861	861	862	861	862	863	862	857	852	852
Nov ...	865	873	874	875	875	874	874	872	870	870	870	868	867
Dec ..	868	863	863	862	862	863	864	864	864	863	863	863	862
Means	877	884	885	884	884	884	884	883	883	882	881	879	877
						Summer.							
April ...	875	898	897	896	896	895	894	893	893	891	888	884	879
May ...	878	898	897	896	896	898	898	899	897	892	885	878	873
June ...	883	894	892	892	891	892	894	892	891	889	883	876	873
July ...	893	905	905	903	903	902	904	903	902	900	895	893	889
Aug ...	883	898	807	895	895	896	897	897	896	804	889	887	886
Sept ...	830	853	852	851	850	850	850	851	851	849	846	840	837
Means	874	891	890	880	889	889	890	889	888	886	881	876	873

Table VI —Diurnal Inequality of the

Hours	Mid.	1	2	3.	4.	5	6.	7.	8	9.	10	11.
						Summer Means.						
	+ ·00003	+ 00002	+ ·00001	+ 00001	+ 00001	+ ·00002	+ ·00002	+ ·00001	− ·00002	− ·00007	− ·00011	− 00015
						Winter Means.						
	+ ·00001	+ ·00001	+ ·00001	+ ·00001	+ ·00001	·00000	·00000	·00000	− 00001	− ·00002	− ·00004	− ·00000
						Annual Means.						
	+ ·00002	+ ·00002	+ 00001	+ ·00001	+ ·00001	+ ·00001	+ ·00001	·00000	− 00001	− ·00004	− ·00008	− ·00010

NOTE.—When the sign is + the

for Temperature), as determined from the selected quiet Days in 1898.
Year = 0·43885)

Noon.	1	2	3	4	5	6	7	8	9	10	11	Mid.	Succeeding noon.
						Winter							
897	898	902	901	901	901	900	900	900	900	899	899	899	897
897	899	901	904	905	905	906	905	905	904	902	902	902	892
889	892	896	898	901	902	903	904	904	905	904	903	903	899
853	855	859	863	864	863	863	862	862	862	862	862	861	850
869	873	876	877	877	878	877	877	877	875	874	874	874	869
862	864	867	866	865	866	865	865	864	863	863	864	864	859
879	880	883	885	885	886	886	885	885	885	884	884	884	878
						Summer							
876	878	886	892	897	900	903	903	902	900	899	898	897	875
874	879	887	895	901	903	*904	904	902	901	900	901	900	868
873	880	884	889	893	897	897	898	898	895	894	893	893	856
888	892	899	906	911	914	915	914	913	912	909	907	906	878
885	885	891	896	902	904	904	902	902	902	901	899	897	887
836	838	845	852	856	856	855	855	855	853	851	851	849	837
872	875	882	888	893	896	896	896	895	894	892	892	890	867

Vertical Force as deduced from Table V.

Noon	1.	2	3	4.	5	6	7.	8.	9	10	11.	Mid.
						Summer Means						
– ·00016	– ·00012	– ·00006	+ ·00001	+ ·00006	+ ·00008	+ ·00009	+ ·00008	+ ·00008	+ ·00006	+ ·00005	+ ·00004	+ ·00003
						Winter Means						
– ·00006	– ·00003	·00000	+ ·00002	+ ·00002	+ ·00003	+ ·00003	+ ·00002	+ ·00002	+ ·00002	+ ·00001	+ ·00001	+ ·00001
						Annual Means.						
– ·00011	– ·00008	– ·00003	+ ·00001	+ ·00004	+ ·00005	+ ·00006	+ ·00005	+ ·00005	+ ·00004	+ ·00003	+ ·00002	+ ·00002

reading is above the mean.

Table VII.—Hourly Means of tho Inclination, calculated from the Horizontal

Hours	Preceding noon	Mid.	1	2.	3	4.	5.	6	7	8.	9.	10	11.
		67° +					Winter						
1898. Months.	′	′	′	′	′	′	′	′	′	′	′	′	′
Jan	18 8	18·8	18 8	18 8	18 8	18 7	18·5	18·3	18·4	18 5	18 7	19·0	19 0
Feb	18 6	18 2	18 2	18 2	18 2	18·1	18 0	17 9	17 9	17 9	18·1	18 3	18 3
March..	18 9	18 8	18·9	18·8	18 6	18 7	18·6	18·4	18 4	18 5	18·9	19·2	19 3
Oct.....	17·2	16 6	16 5	16 6	16 8	16 7	16·6	16 7	16 8	17·1	17 5	17·7	17·7
Nov. ...	16·9	16·9	16·9	17 0	16·9	16 8	16 6	16·4	16·4	16 6	17 1	17 4	17 3
Dec .. .	16 2	15 8	15·8	15 7	15 7	15 6	15 6	15 6	15 6	15·7	15 6	15·6	15·7
Means..	17 8	17 5	17 5	17 5	17·5	17·4	17·3	17 2	17·3	17 4	17 7	17 9	17 9
							Summer.						
	′	′	′	′	′	′	′	′	′	′	′	′	′
April...	18 7	18 2	18 3	18·3	18 3	18 4	18 4	18 5	18 5	18 8	19·0	19 3	19 1
May....	17 5	17 3	17·4	17 5	17 5	17 6	17 7	18 1	18·7	19 0	19 0	19 0	18·8
June ..	17·8	17 2	17 2	17·3	17 3	17 4	17 5	17·7	17·9	18 4	18 4	18·4	18 1
July....	17 9	17·7	17·8	17 7	17·7	17 6	17·7	18·1	18 5	18 9	19 0	18·9	18 2
Aug ...	17·9	17 0	17·2	17·2	17·3	17 3	17 6	17·8	18 0	18 3	18 5	18·6	18 2
Sept ...	18·1	17·3	17 2	17·1	17·3	17 4	17·5	17 7	17 9	18 2	18 5	18 7	18 4
Means..	18 0	17 5	17 5	17 5	17·6	17 6	17·7	18 0	18 3	18 6	18 7	18·8	18·5

Table VIII —Diurnal Inequality of the

Hours	Mid.				4.	5.		7.		9	10.	11.
					Summer Means.							
	′	′	′	′	′	′	′	′	′	′	′	′
	) 3	- 0 2	−0·2	−0 2	−0·1	0 0	+0·2	+0·5	+0·9	+1·0	+1·1	+0·7
					Winter Means.							
	+0·1	+0 1	+0·1	0·0	0 0	−0·1	−0·2	−0·2	−0 1	+0·2	+0·4	+0·4
					Annual Means							
	−0·1	−0·1	−0·1	−0·1	−0·1	−0·1	0 0	+0·2	+0·4	+0·6	+0·8	+0·6

NOTE.—When the sign is +

and Vertical Forces (Tables III and V) (The Mean for the Year = 67° 17′·6.)

Noon.	1.	2	3.	4	5	6	7	8	9	10	11	Mid	Succeeding noon.
							Winter						
′	′	′	′	′	′	′	′	′	′	′	′	′	′
18·8	18·6	18·8	18·8	18·9	18 7	18 7	18 7	18 6	18·6	18 7	18 7	18 7	18 6
18·4	18 3	18·1	18 2	18 5	18 3	18 4	18 3	18 3	18·2	18 1	18 1	18·1	18 2
19·1	18 9	18 7	18·7	18 6	18 6	18 5	18·4	18·4	18·4	18 3	18 3	18 1	19 2
17·3	17·1	16 7	16 7	16 6	16 5	16 5	16 3	16 3	16 3	16 3	16 4	16·4	17·3
17 0	16 8	16 8	16·8	16·8	16 6	16 5	16 5	16 5	16·6	16 7	16 8	16 7	16·9
15·7	15·6	15·7	15 8	15 6	15 6	15 5	15 5	15·6	15 6	15 6	15 6	15 6	15·4
17·7	17·6	17 5	17 5	17 5	17 4	17·4	17 3	17·3	17·3	17·3	17 3	17·3	17 6
							Summer						
′	′	′	′	′	′	′	′	′	′	′	′	′	′
18·7	18·3	18 3	18 4	18 4	18 2	17 9	17 9	18 0	18 1	18 2	18·1	18 1	18 8
18·4	18 1	17 9	17 8	17 7	17 4	17 3	17 0	16 9	17 0	17 1	17 1	17·3	17 3
17·6	17 4	17 1	17 2	17 2	17 1	17 0	16 9	16 9	17 1	17 1	17 2	17 2	17·3
17 7	17·7	17 6	17 4	17 0	17·5	17 4	17 3	17 4	17·3	17 2	17 4	17 4	17 6
17·8	17·8	17 8	17 7	17 6	17 4	17 0	16 9	16 8	16 7	16 8	16 8	16 8	17·6
17·8	17·3	17 3	17·4	17·4	17 2	17 0	16 8	16 7	16 6	16·7	16 7	16·7	17·2
18 0	17 8	17 7	17 7	17 6	17 5	17 3	17·1	17 1	17·1	17 2	17 2	17·3	17·6

Inclination as deduced from Table VII.

Noon	1.	2.	3.	4.	5	6	7.	8	9.	10.	11.	Mid.
						Summer Means						
′	′	′	′	′	′	′	′	′	′	′	′	′
+0·3	0·0	−0 1	−0 1	−0 1	−0 3	−0 5	−0·6	−0 6	−0 6	−0 6	−0 5	−0·5
						Winter Means.						
′	′	′	′	′	′	′	′	′	′	′	′	′
+0·3	+0·1	0·0	+0·1	+0·1	−0·1	−0·1	−0 2	−0 2	−0 2	−0·2	−0 1	−0 2
						Annual Means.						
′	′	′	′	′	′	′	′	′	′	′	′	′
+0·3	+0·1	0·0	0·0	0 0	−0·2	−0 3	−0·4	−0·4	−0·4	−0 4	−0·3	−0·3

the reading is above the mean.

APPENDIX Ia.

Mean Values, for the years specified, of the Magnetic Elements at Observatories whose Publications are received at Kew Observatory.

Place	Latitude	Longitude	Year	Declination	Inclination.	Horizontal Force C G.S. Units	Vertical Force C G.S. Units
Pawlowsk	59° 41′ N.	30° 29′ E.	1896	0° 21′·3 E	70° 41′·6 N	·16495	·47084
Katharinenburg	56 49 N	60 38 E	1896	9 47·5 E	70 40 0 N	·17811	50765
Kasan	55 47 N.	49 8 E	1892	7 30 8 E	68 36 2 N.	·18551	·47345
Copenhagen ...	55 41 N	12 34 E.	1895	10 35 3 W	68 47·0 N	·17400	44821
			1896	10 29 5 W.	68 45 6 N	·17422	·44824
			1897	10 24 4 W	68 43 8 N.	·17450	·44826
Stonyhurst	53 51 N	2 28 W	1897	18 27·6 W.	68 53 9 N	·17236	·44663
Hamburg......	53 34 N	10 3 E	1896	11 36·7 W	67 38·8 N	·18061	·43921
Wilhelmshaven	53 32 N	8 9 E	1897	12 41 6 W.	67 49 0 N.	·18028	·44213
Potsdam	52 23 N	13 4 E	1897	10 9·7 W	66 36·3 N.	·18775	·43398
Irkutsk........	52 16 N	104 16 E.	1896	2 5 2 E.	70 11 8 N	20139	·55929
Utrecht	52 5 N.	5 11 E	1896	14 9·7 W	67 4·5 N	·18448	·43618
Kew..........	51 28 N	0 19 W	1898	17 1·4 W	67 17 6 N	·18364	·43885
Greenwich*....	51 28 N.	0 0	1897	16 50·4 W	67 7 1 N 67 6 5 N	·18387	·43567 ·43546
Uccle (Brussels)	50 48 N	4 21 E	1897	14 27·3 W	66 19 5 N	·18017	·13145
Falmouth	50 9 N	5 5 W	1897	18 42·2 W	—	·18595	—
Prague	50 5 N	14 25 E	1897	9 21·1 W.	—	·19884	—
St Helier (Jersey)	49 12 N	2 5 W	1898	17 7 9 W.	65 52 5 N.	—	—
Parc St Maur (Paris)	48 49 N.	2 29 E	1896	15 3 9 W.	65 1 6 N	·19685	·42264
Vienna........	48 15 N.	16 21 E	1895	8 36·0 W	63 9 0 N.	·20731	·40951
			1896	8 30 5 W.	63 7·1 N.	·20756	40944
			1897	8 24 8 W	—	·20785	—
			1898	8 20 8 W	—	·20797	—
O'Gyalla (Pesth)	47 53 N	18 12 E	1896	7 46·9 W	—	·21105	—
Odessa†	46 26 N.	30 46 E	1897	4 47 3 W.	62 30 9 N.	·22030	·42372
Pola‡.........	44 52 N.	13 51 E	1897	9 36 6 W	60 28·0 N	22088	·38967
Nice..........	43 43 N	7 16 E.	1897	12 18 8 W	60 15·4 N.	·22318	·39059
Toronto.......	43 40 N.	79 30 W	1897	4 53 0 W	—	·16650	—
Perpignan	42 42 N	2 53 E.	1896	13 55 3 W.	60 5 9 N.	22398	·38948
Rome..........	41 54 N.	12 27 E	1801	10 45·1 W.	58 4·6 N.	·2324	3730
Tiflis	41 43 N	44 48 E.	1896	1 53 7 E.	55 48·1 N.	·25670	·37775
Capodimonte (Naples)	40 52 N.	14 15 E.	1894	9 41 7 W.	—	—	—
			1895	9 37·0 W.	56 37 9 N.	·24007	·36454
			1896	9 32·1 W.	56 37·1 N.	·24040	·36484
			1897	9 26·3 W.	56 31·4 N.	·24075	·36406

* Of the two values of the Inclination and Vertical Force, the first is based on observations with 3-inch dip needles only, the second on combined observations with needles of 3, 6, and 9 inches.

† Inclination and Vertical Force means from six summer months

‡ Inclination and Vertical Force means from five months, January—May.

APPENDIX IA—*continued*

Place.	Latitude	Longitude.	Year	Declination.	Inclination	Horizontal Force. C.G.S. Units	Vertical Force C.G.S Units
	° ′	° ′		° ′	° ′		
Madrid	40 25 N.	3 40 W	1895	16 6·6 W.	—	—	—
Coimbra.......	40 12 N	8 25 W	1896	17 36·8 W	59 40·2 N	·22620	·38662
Washington ..	38 55 N.	77 4 W	1894	3 39 9 W	70 34 3 N	·19979	56646
Lisbon	38 43 N	9 9 W	1896	17 35 9 W	58 11 8 N	·23346	37648
			1897	17 31·6 W	58 8·2 N.	·23385	·37624
			1898	17 27·7 W.	58 7·8 N	·23418	·37660
Zi-ka-wei	31 12 N	121 26 E	1895	2 15 6 W.	45 55 1 N	·32679	·33743
Hong Kong....	22 18 N.	114 10 E.	1897	0 23 3 E.	31 36 9 N.	·36547	·22497
Tacubaya......	19 24 N	99 12 E	1895	7 45 6 E.	44 22 2 N	·33428	32764
Colaba (Bombay)	18 54 N.	72 49 E	1896	0 33 8 E.	20 55 6 N.	·37463	·14826
Manila........	14 35 N.	120 58 E.	1896	0 51 0 E.	16 39 7 N	·37868	·11333
Batavia	6 11 S	106 49 E.	1896	1 22 0 E.	29 29 5 S.	·36768	·20795
Mauritius	20 6 S	57 33 E	1896	9 48 7 W	54 32 3 S	·23913	·33572
Melbourne.....	37 50 S	144 58 E	1896	8 15 0 E.	67 18 3 S.	·23392	·55936

APPENDIX II.—Table I.

Mean Monthly Results of Temperature and Pressure. Kew Observatory.

1898

Months.	Thermometer.								Barometer.*					Mean vapour-tension
		Means of—			Absolute Extremes					Absolute Extremes.				
	Mean	Max	Min	Max and Min	Max.	Date	Min	Date	Mean	Max.	Date.	Min.	Date.	
1898	°	°	°	°	°	d h	°	d. h	ins	ins	d h.	ins.	d. h	in.
Jan.....	43·4	47 1	38 9	43 0	55 0	31 5 A M	27 6	8 2 A M	30 334	30·711	28 11 P M	29·321	1 0 10 A.M & 5 „	249
Feb.	41 2	46 7	36 0	41 4	56 3	1 3 P M	26 0	21 7 „	29 965	30 384	14 10 „	29·204	21 8 A M.	206
March..	40 0	46 5	34 0	40·3	57 4	18 4 „	26 3	30 6 „	29·895	30 272	11 1 A M	29 349	26 8 P M.	194
April...	47·6	56·1	39·3	47 7	64 0	8 3 „	28 7	6 6 „	29·925	30 278	7 10 P M 8 6 A M	29 327	12 1 A M	·247
May ...	52·1	59 0	45 4	52 2	71 7	23 NOON	36 3	13 4 „	29 845	30 373	7 8 „	29 201	12 4 „	·298
June...	57 7	65 4	50 3	57 9	74 6	21 2 P.M	41 4	1 2 „	29 997	30·293	17 8 „	29 463	25 3 P M.	·355
July ...	61·7	70·4	53 0	61 7	80 1	15 5 „	44 1	11 4 „	30 116	30 389	10 10 P M	29 671	23 4 A M	·380
Aug ...	63 9	72 9	55·2	64 1	83 9	22 1 „	47 0	8 1 „ 31 MIDT	30 024	30 331	31 MIDT	29 678	6 8 P M	426
Sept. ..	60 8	71·8	50 2	61 0	88 3	8 2 „	35 1	29 6 A M	30 112	30 420	4 2 & 8 A M	29 574	30 5 A M	375
Oct.....	53 4	58 9	48 1	53 5	67 1	3 3 „	38 0	13 3 „	29 845	30 370	2 8 „	28 744	18 6 „	350
Nov. ...	45 8	50 6	40 0	45 3	60 0	3 11 A M	28 4	22 11 P M	29 860	30·370	18 10 „	28 764	25 3 „	269
Dec. ...	45 5	49 9	40 5	45 2	56 3	4 1 P M.	26 9	31 3 A M	30 088	30 542	22 11 „	29 083	29 4 P M	·257
Yearly Means	51·1	57 9	44 2	51 1	..	....	..	....	30 001	..	....	..	..	·301

* Reduced to 32° at M S.L.

This table has been compiled at the Meteorological Office from values intended for publication in the volume of "Hourly Means" for 1898.

Meteorological Observations.—Table II.

Kew Observatory.

Months.	Mean amount of cloud (0=clear, 10=overcast).	Rainfall *			Weather. Number of days on which were registered							Wind † Number of days on which it was								
		Total	Maximum	Date.	Rain ‡	Snow	Hail	Thunderstorms	Clear sky.	Overcast sky.	Gales §	N	N E	E.	S E	S	S.W	W.	N W	Calm ‖
1898		ins.	ins.																	
January	8·4	0·910	0·295	5	10	..	..	..	1	22	..	1	1	2	5	1	13	5	3	10
February	7·0	1·275	0·225	27	17	2	..	..	1	12	1	5	1	..	..	2	9	7	4	1
March	7·3	1·175	0·300	3	11	4	1	..	2	15	3	11	5	2	1	1	5	5	1	4
April	5·7	1·025	0·270	27	12	..	1	1	5	9	1	..	5	4	2	7	5	4	3	4
May	8·0	2·460	0·370	19	20	..	..	1	..	19	..	6	4	4	1	3	5	5	3	2
June	7·3	1·375	0·345	26	14	..	2	1	..	13	..	7	3	1	..	3	10	4	2	2
July	6·2	0·670	0·210	1	5	..	..	1	4	9	..	6	4	3	..	3	3	6	6	4
August	5·6	1·110	0·555	7	9	..	..	1	5	9	..	3	2	3	2	2	10	5	4	5
September	4·1	0·420	0·235	29	6	..	..	..	8	2	..	1	4	4	1	4	8	5	3	5
October	7·7	3·345	0·815	29	12	..	..	..	2	20	..	..	7	6	1	6	7	1	3	5
November	6·7	2·050	0·400	25	13	1	..		4	12	..	3	1	6	4	5	5	5	1	11
December	7·0	2·405	1·110	6	10	..	..	..	5	16	1	2	..	..	..	6	16	5	2	4
Totals and means	6·7	18·220			139	7	4	5	37	158	6	45	37	35	17	43	96	57	35	57

* Measured at 10 A.M. daily by gauge 1·75 feet above ground.

† As registered by the anemograph.

‡ The number of rainy days are those on which 0·01 inch rain or melted snow was recorded

§ In a "gale" the mean wind velocity has exceeded 45 miles an hour in at least one hour of the twenty-four.

‖ In a "calm" the mean wind velocity for the twenty-four hours has not exceeded 5 miles an hour.

Meteorological Observations.—Table III.

Kew Observatory.

Months.	Bright Sunshine.				Maximum temperature in sun's rays. (Black bulb *in vacuo*)			Minimum temperature on the ground			Horizontal movement of the air.*		
	Total number of hours recorded.	Mean percentage of possible sunshine	Greatest daily record.	Date	Mean	Highest.	Date †	Mean.	Lowest	Date ‡	Average hourly velocity	Greatest hourly velocity	Date
1898.	h. m.		h. m		deg	deg		deg	deg		miles	miles	
January	27 12	11	5 48	7	63	90	22	35	21	11	7·7	32	31
February	68 36	24	8 30	26	82	97	16 26	29	16	21	12·3	35	2
March	92 54	26	9 0	15	85	115	18	28	17	13	12·5	43	24
April	143 54	35	12 30	16	106	119	16	31	17	5	10·8	36	30
May	146 54	31	13 12	7	111	126	14	40	28	1	10·5	32	3
June	166 18	34	14 36	11	121	135	20	45	35	3	10·1	28	25
July	211 42	42	14 48	24	125	141	6	47	34	11	7·7	24	29
August	205 36	46	12 12	12	123	144	14	50	39	8, 9	10·9	34	18
September	210 12	55	10 48	4	117	133	8	43	26	29	7·7	23	29
October	68 18	21	8 36	1	89	109	23	43	29	13	10·2	27	14 22
November	60 0	22	6 24	1	74	101	5	34	19	23 30	8·7	31	2
December	51 6	21	6 12	23	67	84	4	34	17	23	12·4	50	27
Totals and Means	1452 42	31	..	..	97	..	..	38	..	..	10·1	..	..

* As indicated by a Robinson's anemograph, 70 feet above the general surface of the ground, the original factor 3 being used.

† Read at 10 A.M., and entered to previous day. ‡ Read at 10 A.M., and entered to same day.

APPENDIX III.—Table I.

RESUL OF WATCH TRIALS. Perfo an e 50 Watches which obtained the highest number of marks during the year.

	umber of watch	Escape ance spring, &c.	Mean daily rate					Mean variation of daily rate ±	Mean change of rate for 1° F.	Difference between extreme gaining and losing rates	Marks awarded for			Total Marks
			Pendant up.	Pendant right	Pendant left	Dial up	Dial down				Daily variation of rate	Change of rate with change of position	Temperature compensation	
											0—40	0—40	0—20	0—100
			secs	secs	secs	secs	secs	secs	secs	secs				
S Y Coventry	76152	S r, g b, s o, "Karrusel"	+5·8	+6·0	+6·1	+6·2	+6·5	0·4	0·03	3·7	32·0	39·3	17·9	89·2
Baume & Co, London... ..	03037	G b., s o, 'Tourbillon" chr ometer	−1·7	−2·2	−2·2	−1·1	−0·6	0·3	0·04	3·2	34·0	37·7	17·3	89·0
Fridlander, Coventry ..	25569	S r, g b, s o, "Karrusel"	+1·9	+2·2	+2·5	+2·4	+2·6	0·4	0·04	3·2	32·0	39·0	17·6	88·6
Montandon-Robert, Geneva	1079	D r, g b, s o ..	−1·6	−2·9	−2·7	−4·3	−1·3	0·3	0·02	5·2	33·4	36·6	18·5	88·5
S Smith & Son, London	899-1	G b., d o, pocket chronom Karrus	+1·4	+0·6	+0·1	+2·2	+1·2	0·3	0·04	4·0	33·1	37·6	17·4	88·1
Montandon-Robert, Geneva	1097	D r, g b, s o seconds chro raph	+2·0	+1·6	+2·1	+0·2	+4·5	0·5	0·04	7·0	34·4	36·1	17·5	88·0
Fridlander, Coventry	25570	S r, g b, s o, "Karrusel'	+0·9	+0·9	+1·3	+2·9	−0·6	0·3	0·04	4·7	33·9	36·8	17·3	88·0
E Flinn, Coventry .	18213	S r, g b, s o, "Karrusel '	+3·3	+3·0	+3·1	+4·4	+5·2	0·4	0·02	4·0	32·2	37·0	18·5	87·7
Baume & Co, London . .	03031	G b, s o, ' Tourbillon' chro ometer	−2·2	−0·9	−0·6	−1·9	−2·7	0·3	0·06	4·5	33·9	37·1	16·2	87·2
Montandon-Robert, Geneva	1102	D r, g b, s o, minute chrono ph	−0·8	+1·7	−0·6	−0·2	+1·6	0·3	0·04	4·5	33·4	36·5	17·3	87·2
Baume & Co, London ..	03038	G b., s o, "Tourbillon" chro neter	+2·3	+1·7	+2·3	+2·8	+3·9	0·4	0·05	4·7	32·7	37·6	16·9	87·2
W. Matthews, Coventry	36679	S r, g b, s o, "Karrusel" .	+0·7	+0·5	+0·9	−0·1	+0·9	0·4	0·06	4·8	32·0	38·7	16·3	87·0
S Yeomans, Coventry ..	76673	S r, g b., s o, ' Karrusel"	−1·2	−0·7	−1·1	−0·7	−0·2	0·4	0·06	3·8	32·1	38·7	16·1	86·9
W. Matthews, Coventry	95200	S r, g b, s o, "Karrusel"	+1·0	−0·8	+0·4	−0·4	−0·4	0·3	0·03	6·0	34·3	37·6	14·6	86·5
W Vasel, London ...	1799	S r, g b, d o .	−3·3	−4·3	−2·4	−1·5	−2·1	0·4	0·03	5·2	31·7	36·5	17·9	86·1
J Adams, Coventry ..	6315	S r, g b, s o, ' Karrusel	−4·5	−5·0	−4·9	−1·5	−2·9	0·4	0·04	5·0	32·8	35·1	18·1	86·0
S Yeomans, Coventry .	76689	S r, g b, s o, "Karrusel '	−0·5	[illegible]	+0·6	−0·1	−2·8	0·4	0·03	5·2	31·4	36·6	17·8	85·8
Carley & Co, London	50103	S r, g b, s o "Karrusel"	+1·0	+0·2	−1·5	+0·3	+1·5	0·5	0·04	4·3	30·7	36·9	17·5	85·1
J White & Son, Coventry	35928	S r, g b, s o	−1·0	+1·5	−2·5	−1·0	−1·2	0·5	0·03	7·2	30·1	36·2	18·3	84·6
Usher & Cole, London	29278	S r, g b, d o, "Karru el"	−2·1	−2·8	−2·1	−1·7	−1·4	0·6	0·04	4·2	28·5	38·6	17·3	84·4
Fridlander, Coventry ..	14682	S r, g b, s o ..	+1·2	−0·7	−2·0	+1·1	+0·9	0·4	0·04	4·7	31·5	35·2	17·5	84·2
Kellie Liverpool	8716	S r, g b, s o, "Karru .	+2·5	+3·3	+3·4	+1·5	+2·2	0·5	0·05	5·7	30·0	37·5	16·5	84·0
ridlander, Coventry .	1472	S r, g b, s o	−1·8	−1·1	−5·0	+0·7	−0·4	0·4	0·02	8·0	31·0	34·0	18·9	83·9
Thorneloe, Coventry ...	167	S r, g b, s o, ' Karru	+2·3	+1·0	+1·2	+5·2	+2·8	0·5	0·02	6·0	30·1	35·2	18·6	83·9
me & Co, London	[illegible]	D r, g b, s o, minute c ogra	−3·0	−8·2	−3·0	−2·6	−1·2	0·4	0·02	5·5	32·3	32·5	18·9	83·7

Table I—*continued*

Watch deposited by	Number of watch	Escapement, balance spring, &c	Mean daily rate: Pendant up	Mean daily rate: Pendant right	Mean daily rate: Pendant left	Mean daily rate: Dial up	Mean daily rate: Dial down	Mean variation of daily rate ±	Mean change of rate for 1° F	Difference between extreme gaining and losing rates	Marks awarded for: Daily variation of rate 0—40	Marks awarded for: Change of rate with change of position 0—40	Marks awarded for: Temperature compensation 0—20	Total Marks. 0—100
			secs	secs	secs	secs	secs	secs	secs	secs				
P and A Gave, London	12648	S r., g b, d o ..	+1·2	−2·2	−0·2	−2·0	+0·8	0·5	0·03	5·2	30·8	35·1	17·8	83·7
C J H Marlow, Coventry	20439	S r, g b, s o, "Karrusel"	+6·5	+6·2	+7·0	+3·3	+9·0	0·5	0·02	7·7	30·0	34·7	18·9	83·6
C J Hill, Coventry	149909	S r, g b, s o, "Karrusel"	−0·5	+0·5	+1·2	+0·4	−1·3	0·6	0·01	5·8	27·3	36·8	19·3	83·4
Usher & Cole London	29273	S r, g b, d o, "Karrusel"	−1·2	−1·7	−2·3	−2·6	−1·4	0·5	0·06	5·5	29·4	38·0	15·9	83·3
Montandon-Robert, Geneva	1101	D r g b, s o, repeater and chronograph	+1·6	+0·5	−1·2	+3·9	+2·1	0·4	0·07	4·5	31·4	36·4	15·4	83·2
S Smith & Son, London	191–222 25112	S r, g b, s o, "Karrusel"	+4·9	+4·2	+4·5	+1·1	+4·7	0·5	0·04	7·2	30·6	35·5	17·1	83·2
Usher & Cole, London ..	29274	S r, g b, d o, "Karrusel"	+1·5	+1·1	+1·4	+1·4	+5·1	0·5	0·03	6·7	29·8	35·2	18·2	83·2
W Matthews, Coventry	36449	S r, g b, s o, "Karrusel"	−0·3	−0·8	−0·4	−1·9	+0·3	0·6	0·04	5·5	28·0	37·7	17·5	83·2
Wales & McCulloch, London .	3395	D r, g b, d o	−1·4	−0·7	−4·4	−1·4	−2·2	0·4	0·06	5·0	31·5	35·9	15·7	83·1
J White & Son, Coventry	36123	S r, g b s o	−5·8	−2·2	−5·0	−1·7	−0·9	0·4	0·01	5·2	31·0	32·7	19·2	82·9
W Matthews, Coventry	36451	S r, g b, s o, "Karrusel"	+0·9	+0·6	−0·4	+2·6	+2·4	0·5	0·05	4·2	29·7	36·4	16·7	82·8
S Yeomans, Coventry	76674	S r, g b, s o, "Karrusel"	+3·7	+4·1	+4·1	+3·0	+6·3	0·6	0·04	4·8	28·5	36·7	17·6	82·8
R Thorneloe, Coventry	2734	S r, g b, s o, "Karrusel"	+0·8	+0·9	+3·1	+2·2	+0·6	0·5	0·07	6·0	29·2	38·0	15·5	82·7
J Player & Son, Coventry	29675	D r, g b, s o, "Karrusel"	+3·6	+3·0	+3·3	+5·1	+4·2	0·3	0·11	6·0	33·3	37·4	11·7	82·4
W Matthews, Coventry	36452	S r, g b, s o, "Karrusel"	−3·0	−1·9	−5·1	−5·9	−2·3	0·5	0·01	6·3	29·4	33·9	19·1	82·4
J Adams, Coventry ..	6955	S r, g b, s o, "Karrusel"	−0·6	+0·4	+0·3	−3·2	−1·4	0·5	0·03	7·5	29·1	35·5	17·7	82·3
S. Yeomans Coventry .	76681	S r, g b, s o, "Karrusel"	+1·1	+0·7	+1·4	+2·2	+3·6	0·4	0·09	6·0	31·7	36·5	14·0	82·2
Newsome & Co., Coventry	131071	S r, g b, s o, "Karrusel"	−0·9	−1·0	−0·2	−1·2	+0·2	0·6	0·05	6·0	27·5	38·0	16·7	82·2
H Williamson, Limited, London	54078	S r, g b, s o, "Karrusel"	−2·9	−3·7	−3·7	+0·4	+0·3	0·5	0·07	6·0	29·9	36·7	15·5	82·1
Newsome & Co, Coventry .	129438	S r, g b, s o, "Karrusel"	+1·5	+1·4	+1·2	+3·0	+0·4	0·6	0·05	5·2	27·6	37·6	16·8	82·0
Carley & Co, London	50165	S r, g b, s o, "Karrusel" ..	+0·8	+1·2	+1·6	+1·2	+1·1	0·6	0·05	6·8	27·6	39·2	15·0	81·8
C J Hill, Coventry	149910	S r, g b, s o, "Karrusel"	+0·1	−0·2	+0·4	+1·9	+2·3	0·5	0·06	5·7	29·5	36·2	15·9	81·6
P and A Guye, London	7296	D r., g b, s o	+2·4	+1·4	+3·1	+3·1	+2·6	0·5	0·08	5·0	29·2	38·0	14·4	81·6
S Yeomans, Coventry	76150	S r, g b, s o, "Karrusel"	+0·3	+0·5	+1·5	+1·8	+1·6	0·6	0·08	6·5	28·7	37·7	14·9	81·3
C J. Hill, Coventry	150010	S r, g b s o, "Karrusel"	+4·3	+5·4	+5·1	+3·3	+3·2	0·6	0·04	5·2	27·3	36·8	17·1	81·2

In the above List, the following abbreviations are used, viz.—s r. for single roller, d r for double roller, g b for going barrel, s.o for single overcoil, d.o for double overcoil, + for gaining rate, − for losing rate

Table II.

Highest Marks obtained by Complicated Watches during the year

Description of watch	Number	Deposited by	Marks awarded for Variation	 Position	 Temperature	Total marks.
			0—40	0—40	0—20	0—100
Minute and seconds chronograph, minute repeater, and perpetual calendar, with phases of the moon	24987	S Smith and Son London	27·6	29·6	13·3	70·5
" " " "	33349	Weill and Co, London	26·3	30·0	12·6	68·9
Minute chronograph and minute repeater	1101	Montandon-Robert Geneva	31·4	36·1	15·4	83·2
" " "	7464	Army & Navy C S, London	27·2	32·2	15·5	74·9
" " ", (and split seconds)	2262	H Golay London	26·4	35·0	13·4	74·8
Minute and split seconds chronograph	12535	Baume and Co, London	29·8	31·0	17·3	78·1
" " "	3320	H Golay, London	26·0	34·4	14·8	75·2
" " "	107962	Baume and Co, London	28·4	34·9	11·6	74·9
" " "	30161	Pembroke Coleman London	24·7	34·4	15·5	74·6
Minute and seconds chronograph	1102	Montandon-Robert, Geneva	33·4	36·5	17·3	87·2
" " "	246988	Baume and Co, London	32·3	32·5	18·9	83·7
" " "	121964	W Russell, Glasgow	22·5	36·8	17·7	77·0
" " "	115749	Stauffer, Son, and Co, London	25·7	36·0	14·8	76·5
Ordinary seconds chronograph	1097	Montandon-Robert, Geneva	34·4	36·1	17·5	88·0
Minute repeater	2314	H Golay, London	28·4	29·9	17·0	75·3

Table II—*continued.*

Description of watch	Number	Deposited by	Marks awarded for			Total marks.
			Variation	Position	Temperature	
			0–10	0–40	0–20	0–100
"Non-magnetic"	192 B 292 25572	S. Smith and Son, London	25·1	36·1	18·4	79·6
,,	192 A 291 25571	,, ,,	25·9	36·5	16·5	78·9
,,	02224	,, ,,	27·3	32·4	17·1	76·8
,,	189–249 25541	,, ,,	28·5	34·4	12·8	75·7
,,	35936	J. White and Son, Coventry	25·6	35·6	14·2	75·4

CROONIAN LECTURE.—"On the Relation of Motion in Animals and Plants to the Electrical Phenomena which are associated with it." By J. BURDON-SANDERSON, M.A., M.D., F.R.S. Received March 2,—Read March 16, 1899.

In a Croonian Lecture which I delivered to the Royal Society in 1867—more than thirty years ago—I exhibited a number of diagrams of graphic records, in evidence of the mechanical relations which I then sought to establish between the movements of the heart and those of respiration in the higher animals.

I have to-day to bring before you results which have also been obtained by a graphic method, which however differs from the other in that the records are written by light, and not by pen on paper, that the time taken in recording is measured in thousandths of seconds, not tenths; and finally, that the events recorded are not the movements of the chest or heart, but the electrical changes which, as will be shown, are found to associate themselves with all manifestations of functional activity in living organisms, whenever these take place under conditions which admit of their being investigated.

Our purpose is to consider the relation of two coincident and concurrent processes, with reference to which we make at the outset the assumption that one is functional, the other concomitant, using the word "function" in the biological sense to imply the doing by an organ of the work for which it is adapted. In the observations which I have made from time to time during the last twenty years relating to the electrical phenomena of plants and animals, it has always been my endeavour to regard them exclusively in relation to the functional activity of the structures in which they manifest themselves. In investigating the function of a living organ or organism, you have to do with a machine that you cannot take to pieces, and it is often the best way to observe how, after its action has been arrested, it goes on again. To do this under experimental conditions is one of the most frequently used methods of the physiologist. The possibility of employing it depends on the circumstance that all animal organisms, and certain parts of plants, possess the faculty of being awakened from a state of rest to normal activity.

Even under the most favourable conditions, the observation of this transition is attended with difficulties which arise from the complexity of the chemical and mechanical changes, and the shortness of the time spent in their accomplishment. It is this crowding together of chemical, thermal, and electrical phenomena into a very brief period which determines the method for their elucidation, a method consisting to a large extent of a determination of *time-relations*, *i.e.*, of the order of succession of phenomena, for it is evident that when you have to do with a number of events which appear to be simultaneous, the most effectual way to determine their causal relations is to ascertain the order of their occurrence. For, inasmuch as one event which follows another cannot be its cause, the proof of their sequence which accurate time-measurement affords may be of infinite value in indicating where the starting point in a complex series of changes is to be sought for

The inquiry as to the relation between functional activity and the electrical phenomena accompanying it, can only be entered upon by finding instances in which both processes can be observed together Amongst these, those are to be preferred in which the question presents itself in its simplest form, the experimental conditions can be most easily controlled, and the observations can be made with the greatest exactitude

It might at first sight seem desirable to begin by describing the electrical manifestations of functional activity in the simplest organisms and organs There are, however, important reasons for following the reverse order To do so is in conformity with the general rule that a problem can be most easily solved when it presents itself in its simplest form. In the lowest organisms the relation of function to structure, so far from being simple, is necessarily very complex, for functions of the most varied kind have to be discharged by one and the same mechanism, and often in default of any mechanism at all that we can discover, whereas in the higher plants and animals we find for the most part that every kind of work has its instrument, every action its agent It is in the highest organisms therefore that elementary physiological questions must be studied, and it is in them that they have been most studied

Of the elementary vital functions, *motion* was the one fixed upon as the subject of this lecture by its founder Its fitness for our purpose is pre-eminent. Motion, in the physiological sense, is simple, controllable, measurable. It is, moreover, a function of paramount importance as the means by which the animal organism maintains its relation to the external world In the higher animals, *muscle* is the instrument of motion, and therefore claims our consideration. It has, in addition, the advantage of being a structure of which the chemical, thermal, and mechanical properties are better known than those of any other. This advantage applies particularly to the muscles of the *frog*, which on that account, as well as on the grounds which have been the occasion of their being most studied, are to be preferred for our present investigation. What we have first to do therefore this afternoon is to determine the relation between the *electrical* concomitants of muscular action and muscular action itself; but before entering upon it, I must occupy you for a few minutes in stating what is at present most certainly known as to the nature of that action.

When a muscle is roused to activity by the presence of an exciting cause, its mechanical properties suddenly change. It shortens and, if the shortening is resisted, becomes tight. If the resistance is such as cannot be overcome, it tightens without shortening. With reference to this mechanical change, we know that it is dependent on chemical change, and that that change is oxidation. Admitting these propositions, we must necessarily believe that the oxidation is sudden, *i.e.*, explosive, because its mechanical effect (the tension or tautness I have mentioned) attains its maximum at a very short period after the moment at which the process begins.

At ordinary temperature we find that in a whole muscle the tension which is induced by an excitation attains its maximum in about 3/100 second. But if we fix our attention on a single muscular element, *i.e.*, on one of the infinite number of molecular mechanisms by the cooperation of which the mechanical change consequent on excitation is brought about, it can be shown that in each taken separately a much shorter duration than 3/100 second must be assigned to the process of transition. According to Bernstein, less than 1/100 second must be assigned to the chemical process which takes place in a muscular element in response to a single stimulus.*

This chemical process of extreme suddenness is followed without measurable loss of time by the conversion of the chemical energy of the oxidisable material into mechanical energy, which immediately manifests itself either in shortening or in the effort to shorten. The way in which this transference takes place must for the present be left an open question, for, as Professor Engelmann explained in the Croonian Lecture for 1895, the transformation of chemical into mechanical energy consequent on excitation of muscle, though by no means an insoluble, is still an unsolved physical problem. We know how much chemical energy is liberated, we know how much work is done, and how much heat is wasted, but we cannot explain how it happens; it being difficult to suppose that the temperature required for such transformation can exist in living muscle.

The absence of a sufficient physical theory of the origin of muscular force does not, however, deprive the mechanical manifestations of the process of their value as simple, measurable, and controllable indications of functional activity. Whether we take the case in which a muscle strives against a resistance which it cannot overcome, or shortens without resistance, or does both simultaneously, the change of tension in the one case, of form in the other, or of both, are measurable processes of which the time-relations can be ascertained with great accuracy. We are on safe ground therefore in using either change of tension or change of form as a means of estimating the vital activity of muscle, and in fact both are required.

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* See Pfluger's 'Archiv,' vol 67, p. 211, 1897, where Bernstein describes his method of measuring the period of latency As in the method described by me in the 'Journal of Physiology' in 1895, a magnified image of the moving surface of a muscle excited directly is received on a slit, behind which a sensitive plate passes. From the curve so obtained, Bernstein determines the moment at which the rate of expansion increases most rapidly, and regards this as the moment at which the moving force is at its maximum This conclusion, of course, applies to the part of the muscle immediately excited

For every investigation in which muscular function is in question, three points come prominently forward: (1) The moment at which mechanical energy comes into play; (2) the maximum energy displayed, and (3) the time at which that display culminates. As regards the first point, the time occupied before the mechanical response begins was, for many years, believed to be 1/100 second. This estimate was accepted as though on the authority of Helmholtz, but was really based on a misunderstanding of his experimental data. But we now know that the change of form resulting from the action of a single instantaneous stimulus begins in the muscle *element* not later than 4/1000 second after the moment of excitation, and I may be permitted to show you how this result can be arrived at with absolute certainty by the photographic method. (Photograph shown.)

As regards the second and third points, we find it better to measure contractile activity by change of tension rather than by change of form, firstly, because the method of measuring tension is less liable to error, and, secondly, because the process of development of tension is more rapid than the development of change of form. For, although with the exquisite methods we now possess of getting rid of inertia in our recording apparatus, it is possible to measure the shortening with great exactitude, yet it is easier to guard against errors of observation when the other (isometric) method is used.

Having seen how functional activity can be measured, we may advert to the question that principally concerns us this afternoon—the question, namely, whether the electrical phenomena may also be regarded as expressions of functional activity. Assuming for the moment the question to be answered in the affirmative, with what part of our tension curve should we expect the electrical concomitant to coincide? Assuredly with the first and second hundredths of a second after excitation, *i.e.*, with the period of greatest activity of the unknown process by which chemical is replaced by mechanical energy, and, for a reason which I will at once explain, with the very beginning of that process. For, as I have already indicated, the transition does not occur at the same moment everywhere, and inasmuch as the method which we use for the investigation of electrical change takes cognisance only of what happens within an area of a couple of millimetres, we should expect it to occur not at a moment corresponding to the maximum development of tension in the whole muscle, but at the moment at which the transition process is going on with the greatest rapidity in the elements immediately concerned within that limited area. Assuming for the moment that this rapidity is expressible as a measurable electromotive force, we should expect the appearance and disappearance of that electromotive force to be represented, not by a curve resembling the tension curve, but by a curve of the form indicated in the diagram. (Curve P′ in Diagram 1.)

Before losing sight of the mechanical changes which have for the last few minutes been occupying our attention, there are two other points which must be shortly adverted to on account of their bearing on what follows. The one is the terminableness and the cyclical character of the mechanical process. The muscle returns to its *status quo* at a certain time after it has been disturbed, a time strictly dependent on temperature and other well ascertained physiological conditions. We do not know as yet *how* it relaxes, whether it is merely a physical reaction, or whether it is by the intervention of a new chemical process. This is a *quæstio vexata* which for the present must remain open.

The second point is that although the mechanical process is limited in time it is not limited in space. If it were possible to imagine a *continuum* of contractile protoplasm, an excitation once started would go on for ever, *i.e.*, it would be propagated from element to element—in every direction if it were of the nature of cardiac muscle, in two directions only if it were of the nature of skeletal muscle. For this process to take place we suppose that each element excites its neighbour. In each transmission the time lost is almost infinitesimal, yet by summation it acquires a definite value, so that the relation between distance travelled and time occupied can, when the temperature and other conditions are known, be foretold. In so far as each element transmits its state of change to its neighbour without loss it resembles the propagation of light and sound, but the velocity of propagation is of so different an order that the comparison must not be carried too far.

Method of Observation.

We are now in a position to enter on the inquiry which more immediately concerns us. Having the order of the mechanical changes which constitute muscular action before us, it will be our purpose to compare with this order that of its concomitant electrical phenomena. Before I proceed with this comparison it is desirable to say that it should be understood that no reference will be made to electrical theory. We merely derive our modes of observation and of measurement from the exact sciences, and aim at the utmost attainable precision; but the phenomena have their chief interest as outward and visible signs of intimate vital processes, of which they afford us the only knowledge that is within our reach.

We choose as our subject of observation a muscle of nearly symmetrical form—a band of parallel fibres. We explore its electrical state, by a conducting arch containing a galvanoscope, the ends of the arch being in contact with its surface It the muscle is no longer living, the galvanoscope gives no evidence of current If it is living, there is again no current, provided that the two surfaces are in the same physiological state If one is *less* living than the other the fact is indicated by a difference of potential between them, a current *flowing through the galvanoscope from the more living to the less living* Vitality is, therefore, here indicated by difference of potential By vitality we mean nothing more than the *capacity for discharging function.* This capacity diminishes by discharge, *i e*, by activity. Accordingly we find that when, for any reason, the muscular substance at one part becomes more active than the muscular substance at another, the former becomes negative to the latter.

Every observation of the electrical phenomena of muscle (or of any other excitable structure) relates either to the state of capacity for action (called in physiology "rest"), or to the state of action or discharge In either case it consists in comparing the states of two contacts,* *i e*, of two parts to which electrodes of a galvanoscope are applied It is obviously desirable for the investigation of the changes at either, that those which take place at the other should be annulled during the period of observation. On this consideration a rule is based, to the mode of carrying out of which I will advert presently.

Most of the results which I shall place before you were obtained with the aid of the capillary electrometer, of the use of which as an aid to electrophysiological investigations I brought before the Royal Society some instances nearly twenty years ago Its application has since been studied with great completeness by Mr. Burch, to whose skill I am indebted for the instruments which I have used for my work during the last ten years, and more particularly for the one which has enabled me to submit to you the photographic results I am now about to exhibit. These photographs, I need scarcely explain, express the excursions of the meniscus of the mercury column as a sensitive plate moves rapidly past the slit on which it is projected, each upward movement of the image indicating that the surface of contact connected with the mercury has become at that moment positive to the other.

I do not propose to give this afternoon even the shortest description of the instrument, and I should not occupy time in explaining why it answers my purpose so perfectly, were it not that with the exception of Professor Einthoven and Dr. R. du Bois-Reymond the leading authorities on the other side of the Channel, and particularly Professor

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* It may be well to note that the contacts referred to here and elsewhere are made by means of non-polarisable electrodes of the kind originally devised by du Bois-Reymond and always used in physiological work

Hermann, have condemned it as an instrument of which the defects are essential and irremediable. As I have answered these criticisms elsewhere, I need only say here that for the investigation of the order and duration of a rapid succession of electrical changes, such as those with which we are now concerned, the instrument surpasses all others, and that by means of it my colleague, Professor Gotch, has with Mr Burch's aid, successfully photographed phenomena in nerve, of which the very existence could not be demonstrated a few years ago.*

The purposes to which we apply it are (1) for the measurement of intervals of time between electrical changes which succeed each other with great rapidity, and (2) the obtaining an estimate of their relative intensities. The properties which make it so invaluable to us are (1) that it responds to the action of a current promptly, beginning when the current is closed, and indicating every change in its strength or direction without measurable loss of time; (2) that *cæt. par.*, the rate of ascent is proportional to the electromotive force of the current which produces it; and (3) that the instrument can be graduated, and its graduation verified by comparison with instruments of greater precision, and thus used for the measurement of differences of potential of longer duration.

The diagrams 1, 2, and 4 illustrate the bearing of these three properties on the cases we have to investigate. As we shall see, a muscle can be brought into action either by an instantaneous stimulus, by a series of stimuli, or by continuous stimulation. Each of these has its mechanical and its electrical response. I will anticipate so far as to say that the three forms of electrical response correspond to the three forms of mechanical. They correspond to the changes indicated by the black lines in the three diagrams. I will further premise that all known excitatory responses—*all electrical changes which are concomitants of action—may be compared with one of these types*.

Case 1.—Response to a continuous stimulation. A difference of potential comes into existence at the contacts at the time *t*, and persists long enough to produce its full effect on the column. (Diagram 1.)

Case 2.—Series of short continuous stimulations. The column moves in alternately opposite directions. (Diagram 2.)

Case 3.—Response to a single instantaneous stimulation. A differ-

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* Full information relating to the instrument will be found in Mr Burch's work 'The Capillary Electrometer in Theory and Practice,' and his papers in the 'Proceedings' (vol. 48, 1890) and 'Transactions' (A, vol. 183, 1892) of the Royal Society. A very perfect method of recording the excursions of the electrometer photographically and of interpreting the curves was described by Prof. Einthoven in Pflüger's 'Archiv' in 1894, and applied by him to the investigation of the electromotive phenomena of the human heart. It need scarcely be added that the two methods are the same in principle. An important paper has also recently been published by Dr. R. du Bois-Reymond in the 'Archiv f. An. u. Physiol.,' 1898, p. 516.

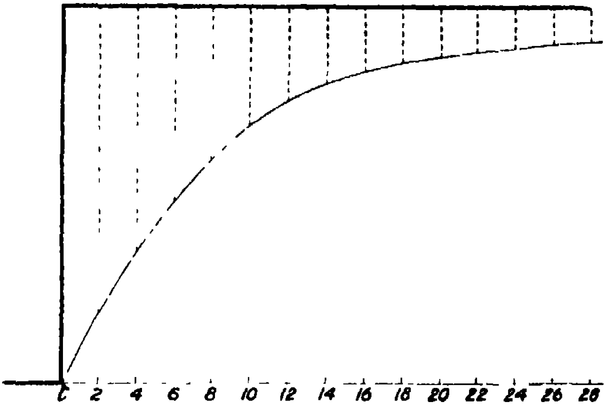


Diagram 1.

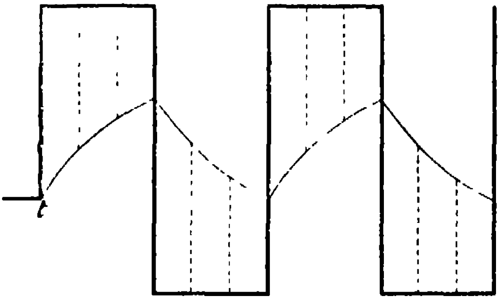


Diagram 2

ence of potential comes into existence abruptly, and subsides abruptly at first, afterwards less rapidly. (P′ in Diagram 4.)

Now I have found that in the study of my experimental results it is of great advantage to proceed *a priori*. Let us assume that there are three types of stimulation, and that each has its form of response. We can best begin by inquiring to which of these three forms the observed variation belongs, and then determine in what respects it conforms with, or differs from, the type.

In the diagrams, I have shown the types of photographic curves which correspond to the three forms of response to stimulation I have indicated. The faint lines represent photographic curves; the strong, variations of potential-difference. In each diagram the strong and the faint lines have been drawn in their true mathematical relation to each other, *i.e.*, so that the vertical *distance apart* of strong from faint is everywhere *proportional* to the *gradient* or slope of the photographic curve, *the proportion being such that if the E.M.F. of the current acting on the electro-*

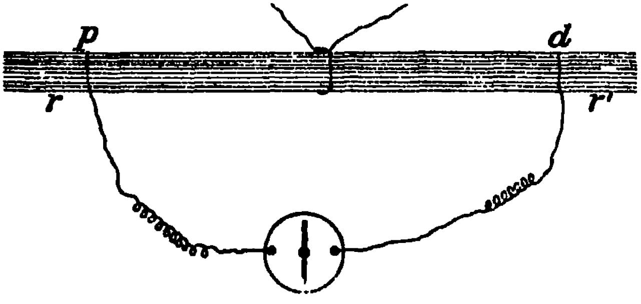
meter varied according to the strong line, the movement of the head of the mercury column would be expressed by the faint line. We shall see as we proceed that one or other of the three forms of photographic curve, which correspond to the three forms of electrical change, just designated as typical, presents itself in every excitatory response we have to investigate, provided that, as I mentioned just now, the changes under one contact only are recorded

To ensure this, the exploring contacts must be so arranged, and the muscle itself so prepared, as to enable us to separate the part of the surface we desire to investigate from the rest, so far as concerns its effect on the instrument we are using as indicator. It is obvious that when we apply our leading-off electrodes to two parts of the surface, both of which are at the same time undergoing change, there must always be a difficulty in determining how far the effect is due to changes at the one or at the other contact It is therefore essential for the correct observation of an electrical change at one of them, that the other should be protected from disturbing influences

The First Fundamental Experiment

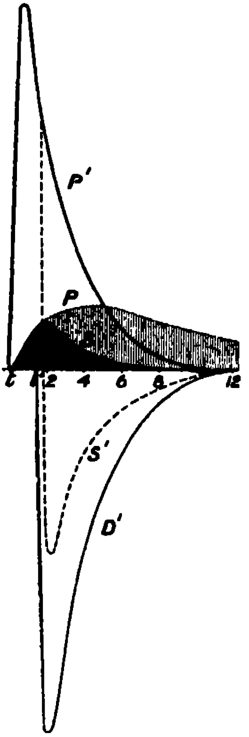
An experiment will show how this may be accomplished. It will also bring us face to face with a phenomenon which is, perhaps, the most fundamental of those which at present concern us, the phenomenon of the wave of excitation, or, to use the designation given to it by its discoverer, the *Reizwelle*. The nature of the experiment is illustrated by Diagram 3, in which the band of parallel fibres represents the sartorius muscle. It is excited (instantaneously) at *r*. A change

Diagram 3



occurs there which is propagated first to the proximal contact *p*, and then onwards to the distal contact *d*, at a rate which in our preparation may be 150 cm. per second. This change is essentially a vital one, but it is attended by a mechanical change represented by the muscle curve, and an electrical change, which we record photographically. Diagram 4 will serve to explain what (as will be immediately seen) actually happens at the moment the wave passes under p It means that a current suddenly appears there, of which the direction is from p to d. When the wave reaches d, a second effect of the same kind (D') occurs, of which the direction is opposed to the first. What the galvanoscopic effect of this must be is easily understood from Diagram 4, in which the two curves P' and D' are placed in a relative position to each other which expresses their time-relation The two effects sum together. In the diagram the curve S' expresses the result of that summation, *i.e.*, the actual variations of difference of potential between the contacts which occurred while the wave was passing from p to d. It will be seen at once why we call this effect the *diphasic variation.*

Diagram 4

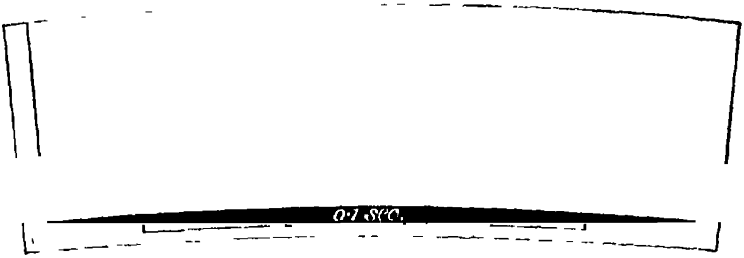


Explanation of Diagram 4 —The horizontal line is that of equipotentiality of the two surfaces of contact p and d The curve P' expresses the relative negativity (negative difference of potential) of the surface p, the curve D' the corresponding relative negativity of the surface d S is a curve of which the ordinates are the algebraic sums of the corresponding ordinates of P' and D'. S is the photographic curve which expresses S', P' is the photographic curve which expresses P. The numbers under the horizontal line indicate hundredths of a second The distance $t\,t'$ expresses the time taken by the wave in its progress from p to d

I explained before, that in accordance with the fundamental properties of our instrument the curve P' would have as its photographic expression the curve P. Similarly the combination-curve S', would have for its photographic counterpart the curve S. May I emphasize the point that if you have the curve P' of a parallel-fibred muscle, you can calculate from it S' and consequently S, but that from S alone you cannot deduce the others In other words, if you know the form of P', you know everything as to the form of the electrical response—the *Reizwelle*.

Let us now take the actual result. As before stated, the two contacts are at p and d, and the muscle is excited at r. The wave affects the muscle first at p then at d, and the consequent movement of the column is photographed (Photo 1).

Photograph 1 [*]



You recognise that it is the counterpart of the deduced curve S. In other words it is the expression of the effects of two similar processes having their seats at the two contacts. Our aim must now be, as I have explained, to annul or suspend the effect of one of them, leaving the other intact. The method is simple. After having obtained the record I have shown you, I tie a fine thread round the muscle between p and d. I tighten the ligature so as to constrict the muscle and again record the variation. There is no change of effect, for the wave is still able to pass the constriction. I tighten again: it still passes I then draw the ends of the ligature hard, and again photograph I find the photographic curve is no longer S but P, *i e.*, it has assumed the characteristic form of the *monophasic* electrometer curve (Photo. 2).

What has the ligature effected? It has exercised no influence on either contact, but it has arrested the progress of the excitatory wave, so that its effect at p only is manifested, and not that at d The relation between the two curves (P and S) is obvious enough when they are seen in succession. It will be still more obvious if I place them on the screen together, in such a way that they are in synchronic relation to each other (Photo. 3).

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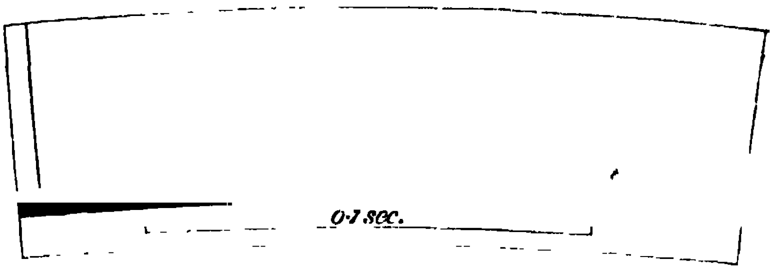
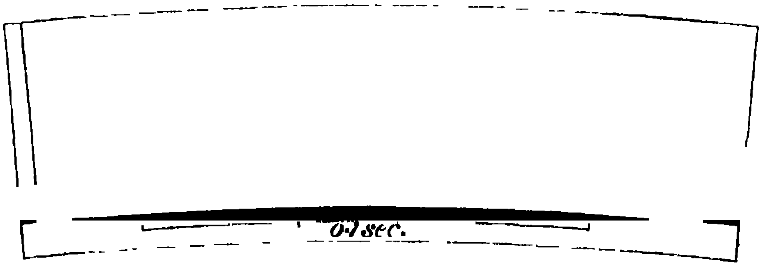
[*] ***Photographs* 1, 2, *and* 3**—**Curarised Sartorius kept for about twenty-four hours in 0·6 per cent. solution of chloride of sodium. Temperature during observation 9° C. Contacts, &c., as in Diagram 5, but p much nearer to d.**

Photograph 2.*



Photograph 3 *



The experiment may be further varied by altering the seat of excitation from *i* to *i'*. You thus obtain a photographic record which represents what happened at *d* in the unligatured muscle. If the muscle is in a normal state, this is an exact reversed counterpart of photograph 2.

If instead of placing the ligature half way between *p* and *d*, we place it close to the distal electrode *d*, the proximal may then be placed in a succession of experiments at different distances from the seat of excitation without altering the form of the recorded variation; the time at which it begins depends in each case on the distance of the proximal contact from the seat of excitation.†

In all of these instances the ligature acts as a *block*. Without interfering with the condition of any other parts it kills the part which it grasps and makes it incapable of transmitting the excited state from the living structural elements on one side to those on the other; but if we compare the condition of the unexcited preparation immediately before and after the application of the ligature, we find evidence that breach of continuity of function is not the only effect produced by it. If the one contact is placed *on* the ligatured part it is found that, irrespectively of any excitation, there exists a large difference of potential between the contacts, which may amount to four or six hundredths of a volt.

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* *Photographs* 1, 2, *and* 3.—Curarised Sartorius kept for about twenty-four hours in 0·6 per cent solution of chloride of sodium. Temperature during observation 9° C. Contacts, &c., as in Diagram 5, but *p* much nearer to *d*

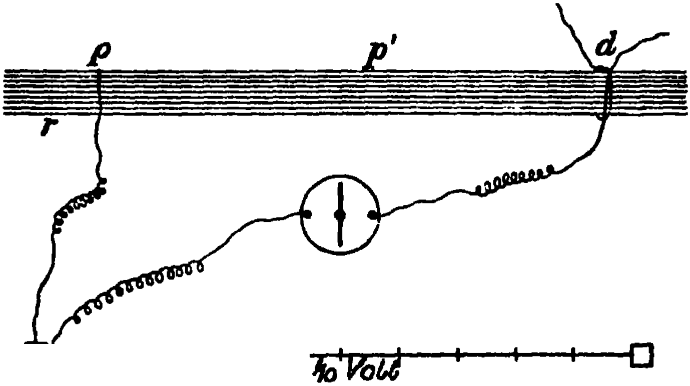
† An experiment of this kind is by far the most exact method which we possess of measuring the conduction-rate in muscle. This rate is most correctly expressed by the quotient $\frac{\text{Difference between the distances}}{\text{Difference between the times}}$ as measured in two experiments in which the distances are different.

The Muscle Current

Now it is easy to prove that this difference is not due to breach of continuity, for if you shove the electrode away from the ligature in either direction it disappears. The phenomenon which is thus brought to light is that to which the great founder of animal electricity, du Bois-Reymond, applied the term "muscle-current," and when the method I have described is employed, it presents itself in its utmost simplicity—for by the act of tightening the ligature previously applied under an electrode, you at once bring into existence a state of things in which the constricted part is negative to the living parts on either side

What happens in this case? What is the difference between the state of the surface of contact immediately before and immediately after the tightening of the ligature? Nothing more can be said than that a certain process which was going on there and which provisionally we call "life," being ignorant of its nature, has been annulled What we actually observe may be represented diagrammatically thus:—

Diagram 5



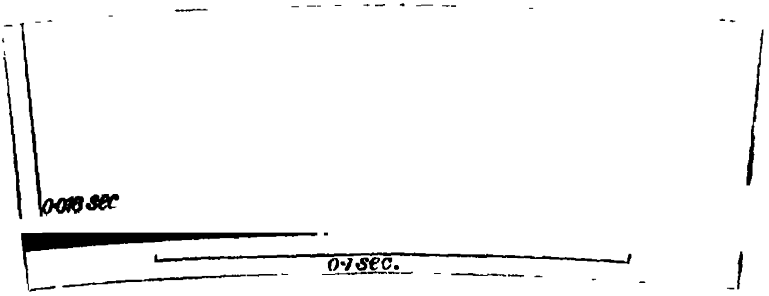
The divided line represents the graduated wire of a potentiometer; at *d* is a ligature as yet not tightened round a muscle, *p* and *d* are equipotential. The galvanometer is at zero and the slider of the potentiometer is up to the block The ligature is tightened; at once the needle indicates a current directed from *d* to *p*, but can be brought by the slider again to zero.

The contacts are as shown in the diagram. Before tightening the ligature between them they are equipotential, because they both rest on muscle in the same physiological state. I represent the electrical concomitant of that state by an arrow, by which I mean nothing more than that if it were possible to connect *p* with some other part of the muscle, without passing through another electromotive surface, there would be a current in that circuit from *p* to the galvanometer. But inasmuch as the actual circuit passes through *d* where the same conditions exist as at *p*, but opposed in direction, there is no current. If by tightening the ligature I annul the effect of *d*, the effect of *p* comes into evidence This statement is simple, and seems to arise naturally from the observed facts, but cannot be received without question, for it suggests that what we call the "demarcation current" has its seat, not at the surface of demarcation, but at the living surface, so that we should have to consider the state of "*Stromlosigkeit*" not as a state of electrical inaction, but as a state of balance.

A similar question would arise as regards the response to excitation. For when, as we have seen, the *Reizwelle* passes under the proximal contact (Exp 1), what happens there (during the 100th of a second that it is passing) is analogous to what I have just described as the effect of suddenly tightening a ligature at that spot. The moment before excitation a state of balance existed between *p* and *d* As the wave passes under *p* it upsets that balance by annulling the outgoing current, then pursues its course until it is extinguished by the ligature From the moment that the tail of the wave has left the edge of the surface of contact behind, it has no action whatever on the indicating instrument We have evidence of this in the curve of variation itself, for the form of the curve is the same whether the wave is blocked by the ligature at one centimetre from the point of observation, or at three, which could not be the case if, as I once imagined, something happened at the moment of extinction.

The complete proof that this is so, is however obtained by another form of experiment in which the seat of excitation (*i*) is shifted from the proximal side of *p* to the proximal side of *d* The unligatured and therefore equipotential muscle is excited in the two positions successively. The results show (1) that the excitation wave is propagated in both directions, and (2) that the form of the curve varies according to the order in which the electrodes are reached. This having been determined, the progress of the wave is stopped by a ligature under the distal contact *d*, and the excitations in the two positions repeated. It is now seen that the form of the wave is the same whatever the direction from which it approaches the point of observation *p*. When the excitation is proximal to *d*, it is not now anticipated by a variation at *d*, and there is consequently a long delay (see Photograph 4) during which the electrometer is unaffected. The experiment affords direct

Photograph 4.*



evidence that, although the whole muscle is in circuit, the presence of the wave cannot reveal itself until it is *under the electrode* As regards the action-current therefore, the electromotive source is always the surface of contact of the leading-off electrode with living substance, not the surface of contact between dead and living.

We may now resume our consideration of the form of the propagated monophasic variation, or excitatory wave. It would be easy to prove by the exhibition of numerous photographs of the monophasic variation relating to different muscles, that all have the same characteristic features, indicating that in each muscular element the electrical change culminates from two to six thousandths of a second after excitation, according to the physiological state of the muscle and the time it has been kept, subsiding at first abruptly, afterwards more gradually, so that its whole duration (*i.e.*, to the summit of the electrometer curve) amounts to from two to six hundredths of a second.

The discoverer of the *Reizwelle*, Professor Bernstein, assigned to it a very different duration. "In every element of muscular structure, the variation lasts between 1/250 and 1/300 second, and coincides with the period of latent stimulation" At first sight this statement seems irreconcileable with fact, but it is much less so than it appears to be. We have only to assume that Bernstein's method of estimating a small and transitory difference of potential between two surfaces, was not sufficiently delicate to enable him to appreciate those which exist during the period of decline, and that what he regarded as the duration of the whole variation, was in reality the duration of its summit only. However this may be, it is clear that we may divide the period of variation into two parts, which we may call respectively the initial rise and the decline, of which the latter lasts eight to ten times as long as the former; and that we may regard the first as a period of upset, the second as a period of restoration. Taking *the period of upset* as equivalent to

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* Curarised Sartorius kept for twenty-four hours Seat of excitation between the leading off contacts, 4 mm from *d*, 20 mm. from *p*.

Bernstein's "*negative Schwankung*," we can accept all he says as to its coincidence in time with the moment of greatest intensity of the process by which chemical is transformed into mechanical energy—the moment in the shortening of an unloaded muscle at which *its rate of change increases most rapidly* As regards the period of decline, it might suggest itself that the return of each element to its previous state is in every instance the expression of an anabolic process, not merely a result of the cessation of the opposite process. The facts we are considering, however, lead us for the present to regard the whole variation as the concomitant of one and the same chemical process, and we are confirmed in this view by the observation that, as we shall see immediately, the modifications which the monophasic variation undergoes under external or accidental conditions affect both stages equally

Of these conditions one of the most important is temperature, particularly when muscles which have been kept for some time in physiological salt solution are used * We have hitherto had in view the Sartorius which has been kept for some twenty-four hours, and is at the temperature of about 10° C By placing it in a cooled chamber at a temperature some 6° C. lower, and allowing it to remain there until it has acquired the temperature of its environment, its mode of responding is not changed, but only in its relation to time In shortening, it takes a longer time to attain its minimum length, and if its contraction is resisted, its period of effort is of longer duration. Consequently it is able to do more external work in a *single* effort than before, although it is not able to support a heavier weight or maintain a greater tension in a *continuous* effort Now all these modifications depend, so far as I have been able to ascertain, on diminution of the rate of propagation of the excitatory wave. As has been already stated, we are able to measure this rate with great facility and accuracy. By alternately cooling and warming our chamber we can determine in any number of instances the change of rate which a difference of 2°, 4°, or 6° C. produces, and compare the data so obtained with the effects of the same changes on the duration of the monophasic variation and on that of the mechanical effort which it accompanies.

Photograph 5

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* 'Journ of Physiol.,' vol. 23, p. 332.

Up to this point the phenomena we have had under consideration have been associated with the response of a muscle to a single instantaneous excitation, *i.e.*, the monophasic variation and the momentary contraction which it ushers in. We must now pass on to the consideration of the electrical concomitants of those forms of contraction which more obviously resemble the natural action of muscles

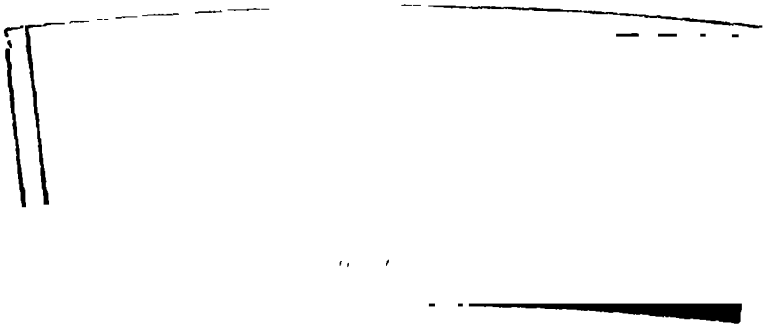
Physiologists have for half a century taught that natural muscular action, whether reflex or voluntary, is made up of single contractions of definite duration, such as those we have been considering, *i.e.*, of a rhythmical series of such contractions of definite frequency. This doctrine—that voluntary motion is a well organised system of twitches—is now commonly expressed by calling it a *tetanus*, a word which was some fifty years ago diverted from its medical signification to be adopted as a technical term in physiology, but not precisely in its present sense. What is now meant by it is that every contraction, however continuous it may appear to be, is in reality discontinuous. This conclusion was arrived at by a method which, though sometimes of great value to the physiologist, does not always lead to the discovery of truth—the method which consists in first imitating a natural process, and then mentally transferring the characteristics of the imitation process to the natural process which it represents In the present instance the study of artificial tetanus has taught us a large proportion of what we know as to the properties of muscle, but not much about voluntary contraction. In assuming the identity of the latter with experimental tetanus, physiologists have perhaps minimised certain fundamental difficulties and assigned undue value to certain analogies.

Of the difficulties, the most obvious one is that discontinuity could not, if it existed, be of any advantage For if we regard the muscular system as the mere instrument of the central nervous system, and every muscular fibre as the instrument of the motor cell which governs it, it is difficult to see how subjecting that muscular fibre to a rhythm of its own could have any other effect than to interfere with its efficiency. Of the analogies the chief are, first, that just as when you listen to a muscle in artificial tetanus you hear a musical sound of which the frequency of vibration corresponds to that of the stimuli, so a muscle when contracting voluntarily gives out the *quasi*-musical sound, which Wollaston compared to the rumble of wheels over pavement. The other analogy relates to the reflex spasm of strychnine, which is not only rhythmical in itself, but is accompanied by a series of electrical changes which are as rhythmical as if they were evoked by a series of stimuli. The discussion of the muscle sound lies outside of our present inquiry: the spasm of strychnine will be considered after we have examined the electrical concomitants of artificial tetanus.

Second Fundamental Experiment.

The point to which I have first to draw your attention is the form of photographic curve which is obtained when the Sartorius, injured under one electrode by a ligature, is excited by a series of stimuli of which the frequency is about 60 per second. The photograph shows that the column rises at first abruptly, but afterwards in such a way that the rate at which it rises is at any moment proportional to its distance from the point to which it will eventually arrive, *i.e.*, to the distance between the corresponding point of the curve and its asymptote. The electrical state, therefore, which comes into existence when a muscle is tetanised (*i.e.*, subjected to a frequent series of excitations) corresponds to diagram 1 In other words, the electrometer is acted on by the same difference of potential between its terminals throughout, with the exception that the effect of the first, or first couple, of excitations is often greater than that of the succeeding ones. Although this hardly needs proof, it can be easily verified by direct experiment. With this view our circuit is so arranged that we can, without altering the resistance, project on to a second photographic plate the effect of allowing a constant difference of potential to act on the mercury column just as the plate is passing behind the slit. On comparing this curve with the tetanus curve they are found to be nearly identical.

Photograph 6 *



Let us now take the case in which a muscle is tetanised in the same way as in the last instance for a succession of periods of one-fifth second, alternating with equal periods of rest (Photo. 7). The complete correspondence of the photographic curve with that represented in

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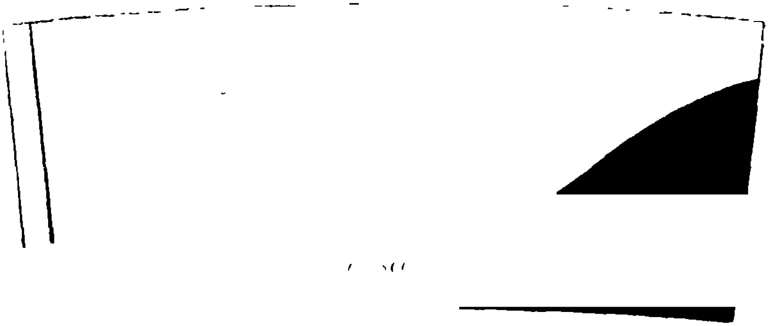
[*] Sartorius not curarised Indirect excitation The undulations on the line of ascent indicate the frequency of the stimuli—60 per second. The radial line indicates here, not the moment of stimulation but that at which the short circuit of the secondary coil was opened

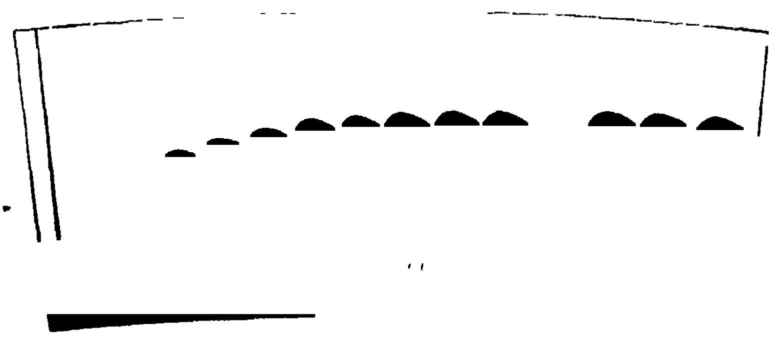
diagram 2 indicates that the conditions correspond with those which are there theoretically represented. During each period of excitation (tetanus) the movement upwards of the meniscus is determined by the difference of potential. During the intervals it follows in its fall the similar curve of depolarisation.

Photograph 7.*



From this we may now proceed to other forms of experimental tetanus in which the excitations are less frequent. Provided that the frequency is not much less than 40 per second, the general contour of the curve resembles the other one, with the exception that the effect of each excitation is seen separately (Photo. 8). If the frequency is diminished to 20 per second the undulations are more ample, while the curve rises to a lower level, the reason obviously being that the electrometer is acted on by a smaller number of excitations in a given time.

Photograph 8.†



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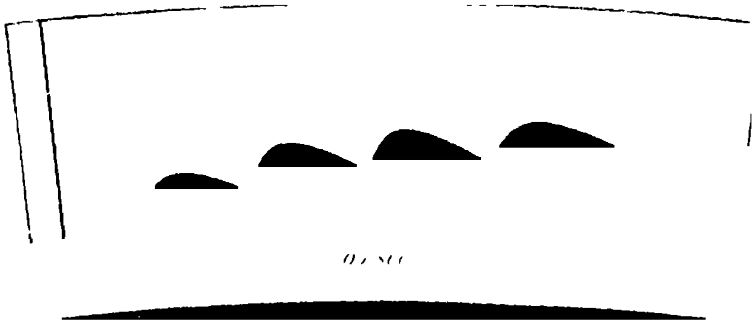
* Frequency of excitation as in Photo. 6. The original shows similar undulations in the ascents, which the copy by inadvertence does not show.

† The first four undulations have been imperfectly copied.

Photograph 9.



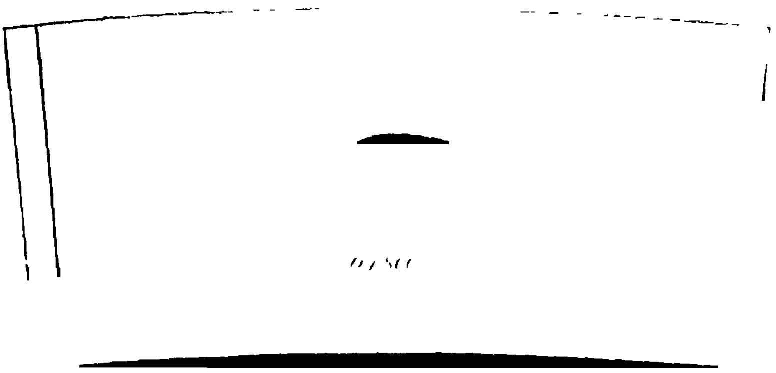
Diminishing the frequency still further (to 14 per second) we obtain curve (Photo. 9) of which the character is that of a series of equal an similar monophasic variations.

The Reflex Electrical Response

We now go on to compare the variation-curve of artificial tetanus with the nearest approach to a normal contraction we can obtain for investigation, viz., the reflex response of the motor apparatus of the spinal cord to an instantaneous stimulation of the cutaneous surface A ligature is applied as before to the tibial end of the Sartorius under the distal contact, but inasmuch as the muscle must now be excited through its nerve, the proximal leading-off contact is on the hilus. The mode of excitation is the same as before, but in this case the effect has first to be communicated to the motor cells of the spinal cord through the sensory apparatus, a process which occupies a relatively considerable length of time. The motor cells then deal with it automatically, responding to it in their own way, and inducing in the muscles under their control an action which is the faithful and exact expression of the changes going on in themselves.

As is well known, it is not possible in a normal preparation to obtain an unfailing response to an instantaneous stimulus applied to the cutaneous surface, but the previous injection of a trace of a strychnine salt (*e.g.*, 1/30 milligram of the sulphate) is sufficient to give to the motor apparatus of the cord the required degree of excitability. A single induction current applied to the skin then evokes in the Sartorius and other muscles, first a twitch which resembles the response of the same muscle to a similar stimulus applied to its nerve; a little later, this twitch is replaced by a short, sometimes thrilling, spasm resembling a short tetanus. What I have to show you is that, although the reflex spasm resembles a short artificial tetanus as regards the way in which the muscle contracts, the contractions are shown by their electrical concomitants to be of a different nature. The strychnine spasm, as it is rightly called, is seen not to be a tetanus, *i.e.*, not to consist of a series of single twitches, but to be a succession of continuous contractions, the rhythm of which depends on the spinal cord, not on the muscle.

Photograph 10*



The grounds on which this conclusion is founded appear to me to be unequivocal. The observation is a simple one. The automatic mechanism, which carries the photographic plate, liberates as before, at the beginning of the period of exposure, an induction current which pricks the skin of the preparation. After an interval which may be about a tenth of a second (during which a *quasi*-psychological process is going on in the spinal cord) the muscle responds. A curve is drawn simultaneously by the writing lever to which the end of the muscle is attached, which indicates that it is in spasm;† but it is the photographic curve which tells us the nature of that spasm. Each ascent of the meniscus is seen to be the response, not to a single instantaneous, but to a short continuous, stimulation, of which the duration can be easily deduced by measuring the time interval between the beginning and the culmination of an excursion. By subjecting the muscle artificially to series of excitations of similar duration with corresponding intervals of inactivity, one can produce an imitation of the strychnine spasm which, both in its mechanical and electrical characters, resembles the natural one (see Photo. 7).

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* Freshly prepared Sartorius attached to pelvis and connected to spinal cord by its nerve. Leading off electrodes on hilus and tibial end. Exciting electrodes applied close together to skin of flank of decapitated preparation.

† The curve is often toothed, the teeth corresponding in frequency with the electrical undulations.

Before leaving the subject of the strychnine reflex, I must refer very briefly to such previous observations as bear on our present inquiry. The phenomenon is of interest as being one which could not have been discovered had we not possessed the capillary electrometer. Its discovery was, indeed, the outcome of the first attempt made by Professor C Lovén to use that instrument for the investigation of the electrical properties of muscle just twenty years ago. He was good enough to make for me the electrometer which was used in some of my own earliest experiments. Shortly afterwards, Mr. Page devised the method of obtaining photographic records of our own results and, amongst others, of those of Lovén relating to the strychnine spasm. Lovén's observation has served ever since as a support for the doctrine of discontinuity. No one would be more willing than he would, if he were with us this afternoon, to recognise its true meaning.

The conclusion to which all the facts we have had before us up to this moment lead, is that normal muscular action is the manifestation of what happens in the motor nervous system. If this motor impulse is so short that we are obliged to call it *instantaneous*, the response is correspondingly brief; if it lasts longer, we call it *continuous*, recognising that the difference between the two is merely one of duration. In either case it is of the essence of the response that it is terminable. There is no difficulty in understanding on teleological grounds why a muscle *must* relax; but of the mechanism by which it is brought about we know little, excepting that it is localised in the muscular structure. Each element—each tagma—returns to its *status quo* in the same way in a curarised muscle as in a normal one; but whether this power of recovery is a process by itself, as some physiologists hold, is a question which is at this moment much debated, but by no means settled. It is only in so far as it relates to the electrical concomitants that it here concerns us. Without prejudice to the question whether, as Fick and Gad maintain, the relaxation of a muscle is dependent on a special chemical process or not, it falls within our present scope to inquire whether by comparing with a normal muscle, one which not only does not relax but has been deprived of the faculty of relaxing, we can arrive at any electrical indication of such a process. Fortunately we have within reach a means by which this experiment can be made

The Continuous Response of a Veratrinised Muscle.

The alkaloid veratrine* is an agent by which a muscle excited by an instantaneous stimulus is deprived of its power of recovering itself. The quantity of the alkaloid required to produce the effect is extremely small. The addition of one part in a million of veratrine to the physiological salt solution in which a muscle has been kept for several hours, is sufficient to give it this property or, as it may be expressed, to "veratrinise" it thoroughly The alteration of the properties of a muscle by veratrine in such a way that it *must* continue an effort once begun, has been long known. It is an example of perfectly continuous contraction. Normal muscular contraction being regarded, as I have said, as discontinuous, the relation between it and the continuous contraction of veratrinised muscle has not been sufficiently considered. When therefore we set to work to measure the maximum contractile effect of a "veratrine spasm," I was both surprised and gratified to discover that the tension of a veratrinised muscle, when excited by a single instantaneous stimulus, was as great as that of a similar but unveratrinised muscle when subjected to a succession of stimuli, *i e*, when artificially tetanised It can also lift as great a load and hold it up for several (10--20) seconds at as great a height (Tracings shown.)

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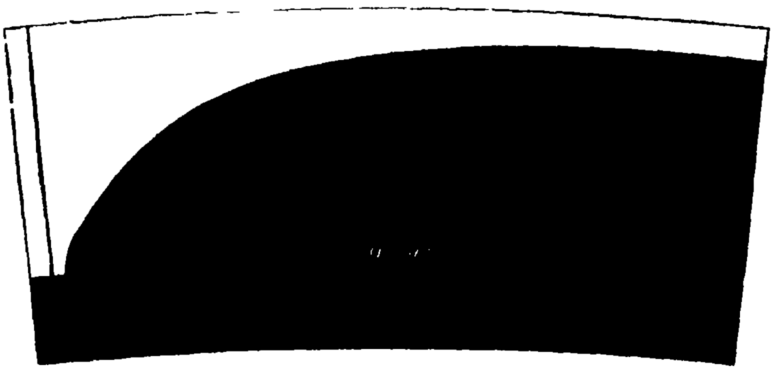
* The veratrine used was kindly prepared by my friend Professor Dunstan, F R S.

We then proceeded to investigate the electrical concomitant of the veratrine "tetanus," if I may so call it (Photo 11) and found it to be identical with that of an artificial tetanus produced by a succession of stimuli of sufficient frequency Its true character can be best judged of by comparing it with Photo 12, which was obtained by introducing into the unchanged circuit a constant difference of potential in the way before explained (p. 54).

Photograph 11.*



The fact that the veratrine spasm has the mechanical and electrical character of a continuous contraction is of value, not from its bearing

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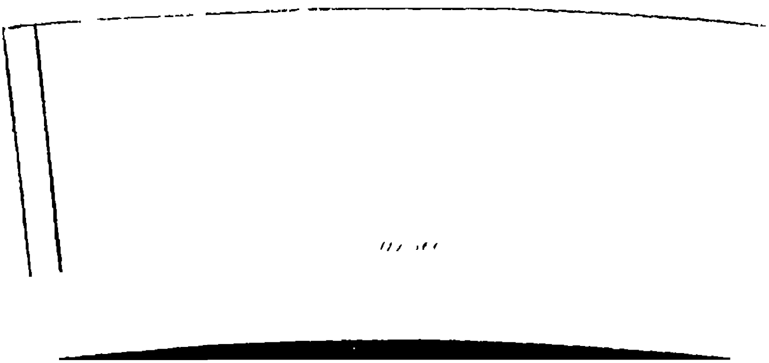
* Electrical response of curarised and veratrinised Sartorius to an instantaneous stimulation. Leading off contacts at middle and tibial end, exciting electrodes near pelvic end. The initial rise of the curve is steeper than that of the comparison curve (Photo 12).

Photograph 12.*



on the mode of action of a particular chemical substance, but from the evidence it affords that discontinuity is not essential to energetic display of contractile force. In this respect it would be wholly irrelevant to object that the data derived from experiments on a poisoned muscle cannot be applied to a normal one All that it is required to prove is that it is possible for a spasm which is not discontinuous to be as effectual for the doing of external work as a normal contraction. It can hardly be disputed that the contraction of a veratrinised muscle is continuous. It is, therefore, no longer possible to assert that discontinuity is essential to functional capacity.

That our results differ from those of other observers is to be attributed to the mode of using the alkaloid, and to the homœopathic minuteness of the dose. We estimate the quantity of veratrine which actually enters the muscle not to exceed 1/10,000 milligram.

The Heart.

We now turn from the skeletal muscles to the organ by the rhythmical contractions of which the circulation is maintained. The mechanical response of cardiac, like that of skeletal, muscle can be evoked either directly or indirectly, but the heart has this peculiarity that each part of it has attributes which we are accustomed to regard as nervous rather than muscular. It has above all the property which belongs, as we have seen from our experiments with strychnine, to the motor cells of the spinal cord—that of discharging itself rhythmically when in a state of continuous excitation. It is characteristic of heart-muscle that it exhibits alternating periods of rest and activity, and we have now the clearest evidence that it is not in virtue of its possessing an intrinsic nervous system that it has this property. In another important respect it resembles the motor apparatus of the cord, namely, that its relations to stimuli are governed by what has been called the "all or not at all" principle. It either does not respond or, if at all, responds completely. In these respects, therefore, the action of the heart is comparable neither with that of muscle acting independently, nor even with that of the muscle nerve preparation, but rather with that of muscle acting under the direction of the motor *neuron* which governs it.

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* Comparison curve obtained by leading off from the compensator a current of E.M.F equal to that of the "action current"; leaving the unexcited muscle in circuit.

I began the investigation of the electrical phenomena of the heart's beat in 1881 with Mr. Page. We made out two new facts, namely, that the electrical change which is evoked by excitation of the surface is propagated, at a rate dependent on temperature, not in one direction only but in all, as Engelmann had already shown to be the case with regard to the wave of contraction; and secondly, that the monophasic variation is not, as had been supposed by previous observers, an instantaneous change, but lasts during the whole period of energetic systole. But neither Mr. Page nor I understood then the nature of the initial "spike," which is so striking a feature in the photographic record of the variation in the *uninjured* heart. For its explanation I am indebted to Mr. Burch, whose investigations on the use of the capillary electrometer for measuring the electromotive force of currents of short duration have been of so much value to physiologists. The moment it was understood that the spike indicated a diphasic variation analogous to that of the muscle, I felt that I had the key to the complete understanding of my own previous observations. I was, moreover, able to bring these into complete harmony with those of Professor Engelmann made about the same time with the rheotome and galvanometer.

Let me ask your attention to the photographic curves of the diphasic and monophasic variations which I have placed one above the other in synchronic relation to each other. It is to be noticed that the movement of the recording surface is very slow, about a centimetre a second only. To obtain the monophasic curve you have to place the distal electrode on a spot which has been devitalised by scorching, and which is consequently physiologically inactive, the proximal electrode on the living surface near the junction between auricle and ventricle. The instantaneous stimulation is applied to the auricle some couple of millimetres distant from the proximal leading-off electrode. The *Reizwelle* is propagated from the auricle to the base of the ventricle and then on to the devitalised spot, so that before it arrives at the contact it is extinguished.* Consequently the change which is expressed by the electrometer-curve takes place exclusively at the proximal contact-surface. It differs only from the monophasic variation of skeletal

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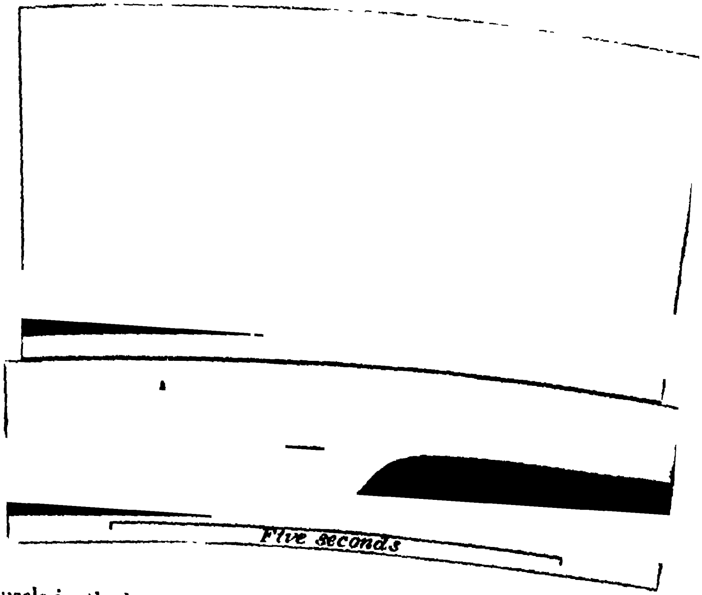
* This mode of observation corresponds to the first fundamental experiment in muscle (see p. 45).

Photographs 13 and 14 *



muscle in the longer duration of the period which intervenes between culmination and decline, and consequently bears a greater resemblance to the effect of a short continuous excitation of muscle than to that of an instantaneous one.

Turning to the diphasic variation obtained when the surface underlying the distal contact is not devitalised, we see that during the whole intervening period just referred to, the two contact surfaces are approximately equipotential. This of course does not mean that both are physiologically inactive, but simply that the influence of the one exactly balances that of the other. This meaning of the diphasic variation is (with the exception of the initial spike) that which was assigned to it in 1882. It results from the mutual interference of two monophasic variations, the dip of the curve at the end indicating that the effect of the distal contact overlasts that at the proximal.

The general result of these observations is that, just as from the mechanical point of view the systole of the ventricle has lately been shown to be entirely analogous to the response of a muscle to an instantaneous stimulus, provided that we substitute volume for length and lateral pressure for tension,† so as regards the electrical phenomena

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* Ventricle of heart of *R. esculenta* arrested by Stannius' ligature. Exciting electrodes on auricle. Leading off contacts at base and apex. In 13, apex surface devitalised by heat; in 14, both surfaces uninjured.

† O Frank, "Zur Dynamik des Herzmuskels," 'Zeits. f. Biol.,' vol 32, p. 370.

there is a complete analogy between the monophasic and diphasic variation of the heart and of muscle, respectively, provided that we bear in mind that the one is a response to a short continuous stimulation, the other to an instantaneous one.

Dionæa.

My last example of motion and its accompanying electrical phenomena I will take from the plant. As everyone knows, there are certain parts of some of the higher plants which respond to stimulation like the motor organs of animals. These instances have been regarded as indications of the close relationship which exists between plants and animals as regards their elementary physiology. The subject attracted the attention of Mr. Darwin in relation to certain insectivorous plants, and it was at his suggestion that the observations to which I am now about briefly to refer, were made. The electrical changes can be most easily studied and appear in the most striking way in the leaf of Dionæa. The leading-off contacts are applied to the opposite surfaces of one lobe of the leaf. In the resting state the one surface is found to be positive to the other. At a certain moment, a hair on one lobe some 10 or 12 millimetres away from the place under investigation, is touched by a camel-hair pencil or excited by an induction current. The surface which was before positive becomes less so, and the curve described resembles, as you see, the monophasic heart curve.

It is not necessary on the present occasion to do more than refer to this typical experiment, by which it was shown for the first time that the migration of liquid, and consequent sudden closure of the lobes on excitation, is accompanied by an electrical change analogous to that in contracting muscle, and that in the leaf this is propagated at a rate varying with temperature. Although the experiment is one of extreme simplicity the method of investigation has not, so far as I know, been pursued by any plant physiologist. The criticisms which were bestowed on it by animal physiologists I was able to answer in my second communication to the Royal Society, and have now the satisfaction to find that the experimental data set forth in that paper are given in full in Biedermann's important treatise on 'Electro-Physiology.'

I have now, though in a very incomplete way, described the phenomena bearing on my subject so far as I have been able to observe them. May I be permitted to submit to you the indications which they seem to me to afford?

In striated muscle the primary effect of every excitation is a process of oxidation having its seat at the excited part. It may be surmised that this consists of two stages, namely, liberation of previously intramolecular oxygen, and actual oxidation. In a single element of muscular structure the duration of this process, when induced by an instantaneous stimulus, must be exceedingly short, and corresponds with that of the excitatory variation; but in the whole organ may last until the development of tension has reached its maximum.

We have further learned that the monophasic variation is a phenomenon of great regularity, and may be taken as the type from which all other forms of response to stimulation may be derived, either by repetition, prolongation, or interference

Although no attempt has been made to settle the question whether the natural contraction of muscle is discontinuous, it has been shown that the electrical phenomena of reflex contraction afford no ground for supposing that it is so The efficiency of the veratrine spasm seems, at least, to justify us in doubting whether discontinuity is an essential quality of muscular contraction

Finally, reasons have been given for thinking that the phenomena known as the "muscle current" and the "demarcation current" are manifestations of processes which have their seat at the surface of contact between electrode and living muscle.

April 20, 1899.

The LORD LISTER, F.R C S , D C L , President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read.—

I "The Physiological Action of Choline and Neurine" By Dr. MOTT, F R S , and Dr HALLIBURTON, F R S

II "On Intestinal Absorption, especially on the Absorption of Serum, Peptone, and Glucose" By Professor E WAYMOUTH REID, F.R.S

III. "Studies in the Morphology of Spore-producing Members. IV. The Leptosporangiate Ferns." By Professor F. O. BOWER, F.R S.

IV. "Note on the Fertility of different Breeds of Sheep, with Remarks on the Prevalence of Abortion and Barrenness therein" By W. HEAPE, M.A. Communicated by Professor WELDON, F.R.S.

V. "Some further Remarks on Red-water or Texas Fever." By A. EDINGTON, M.B. Communicated by Dr GILL, F.R S.

"A Sugar Bacterium." By H. Marshall Ward, F.R.S., and J. Reynolds Green, F.R.S.

Received March 2,—Read March 9, 1899.

In the 'Annals of Botany' for 1897,[*] one of us published a short note on a curious organism—or rather association of organisms—obtained in Paris, and said to have come from Madagascar, where it occurs as "an excrescence on the sugar-cane."

It consists of a bacterium associated with at least one yeast, and grows in saccharine solutions, producing clumps so like the ginger-beer plant[†] that the assumption seemed warranted that we had here a symbiosis of the same kind as that proved to occur there.

Moreover, the general course of events in the use of this body, which is employed to make a fermented effervescing drink from common brown sugar in water, points to the same conclusion.

In moderately strong solutions containing 15 to 20 per cent. of common sugar in water, the clumps referred to induce a powerful fermentation, resulting in the liberation of relatively enormous quantities of carbon dioxide and some acid, the saccharine liquid being thus converted into a not unpleasant acid drink, with some resemblance to lemonade or ginger-beer.

From the fact that this fermentation occurs rapidly when the corked flask is entirely filled with the recently boiled sugar solution, infected with a few clumps of the organism, it is clear that oxygen is not necessary in any quantity.

This conclusion is also confirmed by the observation that if a bottle of soda-water is opened, and a handful of sugar added with a few of the clumps, and at once corked and wired, the pressure of the carbon dioxide liberated during the active fermentation which at once ensues becomes so great in three or four days at 22° C., as to cause danger of bursting the flask; and if a manometer tube with mercury is attached, as described in the paper above referred to,[‡] the bubbles of gas pressed out come off steadily for many days, or even weeks, at ordinary temperatures, until no more sugar is left.

The general resemblances to the well known kephir, also referred to in the previous paper, led one of us to repeat the above experiments with sugar and milk, instead of soda-water, with the result that carbon dioxide came off as before until all the sugar had disappeared, the milk meanwhile undergoing coagulation into clots, but since these clots remained unaltered for weeks or months, this experiment suggests that the organism—unlike kephir—does not ferment the milk itself, but only the added sugar, and that the clots are simply due to the acids liberated as the sugar is destroyed, a conclusion fully borne out by subsequent investigations.

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[*] Marshall Ward, "On the Ginger-beer Plant," 'Annals of Botany,' 1897, vol. 11, p. 341.

[†] 'Phil. Trans.,' B, 1892, pp. 125—197.

[‡] *Loc. cit.*, p. 137.

A preliminary experiment with the ginger-beer plant showed that it also behaves in the same way as regards milk, the fermentation only occurs if sugar be added, and lasts only so long as any sugar remains.

In the cases both of the present organism and of the ginger-beer plant, if a clump be placed in sterilised beer wort, the resulting fermentation gives a frothing liquid with a beer-like taste and smell, and a rapid deposit of yeast occurs. This primary fermentation is very soon finished, and it is abundantly proved by the experiments that this medium favours the yeast—or one of the yeasts—in the clumps, so that we may regard this fermentation as merely a particular case of an impure alcoholic fermentation, such as is got in ordinary brewing.

During these preliminary trials, and with the object of testing whether acidification of the liquid from the first would affect the matter, it happened to one of us to select lemon-juice as the medium in one case, partly to acidify the medium, and partly to add vegetable matter other than sugar The result was somewhat astonishing. The mixture of soda-water, sugar, and lemon passed into violent fermentation in three days, and carbon dioxide came off abundantly, under a pressure of about 12 inches of mercury, and continued to do for many days.

Such a flask started on April 9, 1897, was evolving gas actively on the 12th, this went on without any apparent diminution until May 24, and even on June 24 gas was still coming off, though now under less pressure No sugar had been added in the interval, and the flask, still unopened, remained on a side bench in the laboratory until January 18, 1898 On that date it was opened and examined. It still stood at a pressure of 6 inches of mercury, and gave off gas as soon as the pressure was reduced. The microscopic examination showed that here, again, as in the beer wort, the medium had favoured one of the yeasts, and although the bacterium was discoverable, it was in abeyance, and the compound organism as a whole was not increasing

In another similar case the flask was started on May 28, 1897, and the pressure of the gas evolved was still supporting nearly 12 inches of mercury on April 23, 1898, and again only yeast predominated in the deposit, and examination showed this to be fully alive, it at once renewed its activity when placed in sugar solutions.

These preliminary experiments will suffice to show that we have in this compound of associated organisms an agent or agents capable of setting up very active fermentation in various saccharine liquids, such as ordinary sugar and water, or soda-water, beer wort, milk, and sugar, or an infusion of vegetable substance, such as lemon pulp, and it is clear that the fermentation, though differing in details in each case, always results in the destruction of the sugar, and the production of enormous quantities of carbon dioxide. Obviously, also, these fermentations are anaerobic.

In order to put this last point beyond all cavil, however, we placed a clump of the compound organism into a mixture of a 15 per cent. solution of sugar to which 10 per cent. gelatine had been added, and kept the tubes at 30° C., while the air was pumped out from the fluid mass and pure carbon dioxide allowed to filter in, and this process was repeated four or five times, and the de-oxygenated gelatine, still in an atmosphere of carbon dioxide, was then allowed to set. In a fortnight the solid gelatine in all these tubes had visible submerged colonies throughout the mass, and examination showed these to consist of the bacterium and yeast found in the original clumps.

Similarly, streak cultures on the same sugar-gelatine medium, grew normally on the sloped surface in tubes filled with carbon dioxide, and in these again were observed the same bacterium and the same yeast as had been found predominating in the original clumps.

These cultures—still preliminary in nature and only dealing with the composite organisms of the clumps as a whole—suggested an obvious method for separating at least this prominent bacterium and yeast from the clumps.

Sugar-gelatine, made as before, was infected with a small piece of a clump, rubbed up by a platinum loop in sterile water, and plates made in the ordinary way in Petri dishes; the dishes were then placed under a receiver, attached to the pump and exhausted, and then filled with carbon dioxide, with proper precautions as to the purity of the gas, filtration through cotton-wool plugs, &c., and the cultures put aside in an atmosphere of carbon dioxide at the ordinary temperature. In a week the solid gelatine showed two kinds of colonies, one consisting entirely of a yeast, the other of a bacterium, and closer investigation showed them to be identical with the prevailing yeast and bacterium in the original clumps.

Repeated plate-cultures made in this way gave consistently the same results, and there was no room for doubt that these are the two essential organisms of the clumps, though they are not the only species found in the original material, there being at least one other yeast-like organism, so common that for some time it was thought it must play an essential part. Since this latter—and certain much rarer forms occasionally found—will not grow in the atmosphere of carbon dioxide, however, there can be little doubt that the two anaerobic microbes isolated by the above process are the essential constituents in the fermentations referred to.

Their further separation by means of repeated plate-cultures, as above, was comparatively easy, the yeast especially being readily picked out and further cultivated in sugar-gelatine tubes.

As it is not at present proposed to deal with the yeast, which appears to be a mere variety of *S cerevisiae*, we pass on to the cultures of the bacterium only.

On repeating the plate cultures exactly as before, except the exhaustion and filling with carbon dioxide, it was found that mixtures of the yeast and bacillus grew as well in air as in carbon dioxide. At first it seemed possible that this was because the yeast rapidly consumed the oxygen and so prepared an oxygen-free atmosphere for the bacterium, but further experiments proved that this is not necessary, and that both yeast and bacterium can be grown in air as well as in carbon dioxide or in hydrogen.

The appearance of the separated bacterium on the sugar-gelatine plates is that of circular, raised, dome-shaped, watery-looking colonies, still, like a firm jelly, and lifting as a whole on the needle Each colony is, in fact, a firm zoogloea composed of short rodlets in pairs or chains, the cell-walls of which are so swollen as to furnish the zoogloea jelly The average size of these rodlets is 2—3 μ long by 1 μ thick, though much longer rods and filaments occur in other media

Having once obtained the organism in pure culture, it was, of course, easy to test its behaviour on various media. It is unnecessary to enumerate all the media tried, or to give details of the cultures, which amount to several hundreds, enough that all ordinary media employed by bacteriologists were tried, as well as a long series of special ones devised to meet the suggestions which arose during the course of the investigation.

A striking fact comes out on surveying these cultures, namely, this Schizomycete practically refuses to grow in or on any pabulum devoid of sugar, and, further, only certain sugars are capable of supplying it with its necessary food. No growth at any temperature could be obtained in normal gelatine-peptone media, or in broth, milk, or other animal extracts, *e g.*, serum-agar, such as is used by the animal pathologists.

Gelatine Cultures.

Gelatine, 10 per cent, added to "black sugar" solution* 15 per cent is a capital medium for cultures at 18° C, or thereabouts, and in air, in hydrogen, or in carbon dioxide, the bacterium formed prominent domed colonies looking like drops of stiff gum or gelatine.

This black-sugar gelatine was also used for cultures *in vacuo*. Plates kept under a receiver permanently attached to the pump, going day and night continuously for a week, showed traces of colonies in eight days, and in fourteen days the colonies in the exhausted receiver were as well developed as those in control plates in air at the same (low) temperature.

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* This "black sugar" is a very dark coarse Demerara sugar and probably contains considerable quantities of mineral and other matter.

Streak cultures on black-sugar gelatine with yeast extract developed very rapidly at and near 18° C. Instead of a spreading slimy mass as on agar, these formed dense gelatinous, almost brittle, tear-like drops and streaks standing a millimetre or more high, and curling up off the surface of the gelatine in a most remarkable and characteristic manner.

On saccharonal-yeast-extract-gelatine similar raised streaks were obtained, but less luxuriant than on brown sugar, possibly because the temperature was lower, or only 10 per cent. saccharose was employed, or from lack of minerals

Beer-wort gelatine gave very slight indications of growth, soon stopping, and never attaining anything like these dimensions, the mere traces observed being probably due to saccharose in the wort.

Certain mineral solutions—*e g.*, Kleb's solution—appeared to inhibit the growth in the black sugar and gelatine

We have here gathered together the principal facts of its behaviour on gelatine media, because they seem to bring out clearly that the gelatine itself has little or no part to play in the nourishment of the bacillus: mere traces of superficial liquefaction occur, and several experiments show that it is of no slight importance what is added to the gelatine, *e g*, the surprising results of beer-wort gelatine

Agar.

Agar (2 per cent) made up with black sugar and yeast water rapidly formed large slimy blister-like colonies in four days, both at 25° C. and at 19° C The contrast between these large, slimy, flat, and extended colonies and the small, raised, dome-like, stiff ones, on gelatine was very marked.

Streak cultures on black sugar agar, with yeast extract, grew rapidly as a dull, honey-like slime, spreading all over the surface in two days at 16—18° and at 23—25° C, as well as at 36—37° C.

Agar made up with peptone, &c, was of no use whatever, nor was potato-agar. Even peptone-agar with black sugar proved unsuitable.

Similarly unsuitable was agar made up with yeast extract and saccharose, which had been filtered through porcelain, though a pale dotted streak formed at first at 25, 27, and 31° C.

Agar (2 per cent) made up with 10 per cent. saccharose and yeast extract not filtered through porcelain, on the other hand, was an excellent medium. In twenty-four hours at 30—31° C., a rapidly growing slimy streak had formed, consisting of filaments up to 60 μ long and more, breaking up into segments of all lengths down to 1 5 μ by 1 μ. These showed no sheaths—they had turned slimy—stained well in gentian violet by Gram's method, and were non-motile. At 23° and 20° C the growth was similar but slower.

Wort-agar, however, was unsuitable, cultures parallel with the above, showing very slight growths at any temperature; results quite conformable with previous experience.

Here, again, we must conclude that the agar is of little or no importance, except as a support. The significance of the failure on agar, to which porcelain-filtered yeast extract and saccharose was added, appears significant, and we shall return to this in order to discuss what happens to the yeast extract and sugar when this mode of sterilisation alone is relied on.

Beer Wort

In beer wort (unhopped) the bacterium grows fairly well at first, rapid turbidity following the infection, and a dirty yellowish deposit soon falls, consisting of the flocculent bacterial masses carrying down colouring matter, but the liquid is not viscous, and the deposit scarcely slimy, and growth soon ceases.

When we reflect that beer wort—*i.e.*, malt extract—is usually an excellent medium for the growth of fungi, it is somewhat surprising that this sugar-loving bacterium should do so badly in it. Thinking that the failure might be owing to the kind of sugar in beer wort being unsuitable, we tried adding saccharose, but no obvious improvement was effected. We have seen (p. 69, above) that beer wort and gelatine gave poor results, and we concluded provisionally that some unfavourable substance occurs in wort.

Struck by the success of the preliminary trials with cane sugar, and remembering the alleged original habitat of the organism, it was determined to try the effect of beet: this was done not only because beet is a well-known source of cane sugar, but also because one of us had previously observed in a certain beet disease, that a bacterium very similar to the one under investigation spreads in the tissues of the sugar-beet, and is connected in some way with the disease itself.

Beet Extract

Cold water extract of crushed sugar-beet was found at an early stage of the work to be a favourable medium. In tubes at 25° C., cultures in carbon dioxide rapidly become turbid, and on the third day, a dense slimy zooglœa-like deposit had fallen to the bottom carrying with it the colouring matters, and containing embedded bacteria. The same at 15° C. in carbon dioxide, the growth being simply somewhat slower.

Parallel cultures—from the same tubes—in broth, gelatine, or agar, devoid of sugars, showed no growth either in air or in carbon dioxide, at 15° or at 25° C.

Tubes of raw beetroot, infected with the gelatinous bacterial clumps, showed evident sinking into the tissues in twenty-four to forty-eight hours at 18°, 25°, and 31°, and in six days the depressed area showed collapsed and browned cells under the microscope. A curious white zone surrounded the discoloured patches. The impression gained was that the bacterium draws out the sugary sap and thrives on it: no proof of direct entry into the cell walls could be obtained.*

Cooked, *i.e.*, sterilised, beet gave a mere wet patch at first, but in a week raised gelatinous lumps of the typical kind were formed.

Here, then, we appeared to have proof that it is really the cane-sugar which decides the success or otherwise of the cultures of this gelatinous bacterium, and we entered on what proved to be a very long series of trials with various kinds of sugars to decide this.

Trials with Sugars.

A series of preliminary experiments in which cane-sugar, glucose, and milk-sugar were employed, made up in various ways, soon showed that this bacterium grows far better in cane-sugar than in the others, and better in solutions made up with yeast-water than in such containing mineral salts, asparagin, tartrates, &c.

We, therefore, made series of parallel cultures at various temperatures, in air and in carbon dioxide, and all infected from the same tube, using the following sugars and solutions:—Levulose, pure glucose, cane-sugar, saccharon, lactose, maltose, dextrin, and a body known in Grubler's catalogue as "dextrin-zucker-losung." The sugars were all made up in 10 per cent. solutions, and definite quantities—equal in each case—of the other ingredients added.

Summing up the results of numerous experiments, it was found that no growth occurred in any medium at temperatures of 35° C. and upwards, except in the case of certain agar cultures, where rapid growth occurred at and near 37° C. for a few days only.

Cane-Sugar.

The most striking results were obtained in all cases with cane-sugar, especially in a very pure re-crystallised form labelled "Saccharon," though we also employed ordinary lump-sugar, and a coarse dark brown moist sugar known locally as "Black sugar."

Made up as Mayer's solution, the brown sugar rapidly became turbid and viscous, and a dense gelatinous deposit of bacteria formed below the surface. This occurred even in tubes to which a few drops of absolute alcohol were added—a result not obtainable with dextrin-Mayer.

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* Attempts at infection were made in view of the alleged connection between bacteria and certain diseases of beet and sugar-cane. See, for instance, 'Kew Bulletin,' No. 85, January, 1894, p. 1, and 'Zeitschr. f. Pflanzenkrankh.,' 1897, No. 7, p. 65.

Far better growth was got with cane-sugar and yeast-extract however.

The best growth was got with saccharon (10 per cent.) and yeast extract, where at all temperatures from 16—27·5° to 31° C., the liquid became opalescent and viscous in two or three days, and deposited the typical gelatinous zooglœa at the bottom of the tubes.

The results so far show that only the various forms of saccharose and beet extract (containing this sugar) afford any pronounced growth of this bacterium, the best results being got with pure saccharon and yeast extract, as indicated by the rapid turbidity and viscosity and the clump of gelatinous deposit. Since other sugars seem quite unsuitable, or only induced slight growths expressed as temporary turbidity and flocculent deposits, it may probably be assumed that in "dextrinzucker"* and in "glucose" solutions, where indication of the viscosity and gelatinous deposit occur, traces of saccharose were contained.

Glucose.

Pure glucose and yeast-water encouraged an excellent growth at first, the liquid becoming turbid and a flocculent deposit settling down in a few days. No signs of viscosity appeared, however, and the deposit was quite loose and easily shaken up. The fairly abundant flocculent growth at first led to the expectation that prolonged cultures, or cultivation at different temperatures, might result in the development of the typical sliminess and viscosity, but repeated attempts show that such is not the case. This sugar did not appear to injure the bacterium, for the deposit was alive after three weeks.

Nor would it grow on or in gelatine made up with glucose. The slight growth obtained in certain cases where commercial glucose was used may have been due to admixtures. The total results show that glucose is not a favourable medium.

Levulose (*Fructose*).

Solutions of levulose prepared as the other solutions became slightly turbid in three or four days, and then the bacteria deposited as a very thin layer, no trace of slime or gelatinous matrix being formed, and the impression resulted that either traces of some other sugar must have sufficed for what activity was evinced in ordinary glucose solutions, because no growth worth mentioning compared with the preceding had occurred, or levulose is to a slight extent a food for the organism.

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* This proved to be true of this medium; it consists of dextrin and cane sugar with a little alcohol.

Similar tubes placed in a vacuum also showed no growth beyond the formation of the flocculent deposit, hardly slimy, and easily shaken up into the clear supernatant liquid—a point of constrast of some importance, for in successful cultures in cane-sugar the viscosity of the liquid above is so great that it is very difficult to shake up the deposit, which is also dense and gelatinous.

Repetitions of these cultures gave the same results—a rapid development of a very slight turbidity and formation of a small non-gelatinous deposit which falls and leaves the liquid clear.

Some attempts were made to see if varying the strengths and composition of the levulose solutions would affect the matter—*e.g.*, Mayer's solution made with levulose gave little or no signs of growth at all with the bacterium, though it proved a splendid medium for the yeast.

Curiously enough, a mixture of beet extract and this levulose-Mayer's solution, though it encouraged abundant growth of the bacillus, rapidly becoming turbid and then clearing as the deposit fell, showed no viscosity nor was the deposit slimy, as occurs in beet extract alone.

Even made up with yeast-extract, no viscosity could be obtained nothing beyond the slight flocculence, and it was clear that levulose is at best a poor food material for this bacillus.

Mixed Sugars.

Proceeding from the observation that the bacterium undoubtedly inverts saccharose, cultures were made as follows - Dextrose-yeast-extract and levulose-yeast-extract were mixed, and infected with the bacterium: an excellent growth of the organism resulted, at both 32° and 24° C., but the deposit which resulted, consisted entirely of the non-sheathed bacterium in flocculent masses, and it remains a puzzle why the sheaths are formed in saccharose and not in the two sugars resulting from its inversion The only conclusion seems to be that proportion of constituents has something to do with the matter.

"*Dextrin-Zucker-Losung.*"

Under the above name, Grubler supplies a syrupy sugar in which the microscope shows delicate needle crystals, and—merely in the spirit of trying all experiments—this was tried with the following results:—

Made up with yeast water as a 10 per cent. or 15 per cent solution, the bacterium slowly formed a large slimy deposit in from five to ten or twelve days at 15° and 23°, but not at 35°, in which rods and chains existed in the colourless matrix. The rods measured $1{\cdot}5\mu \times 1\mu$ to $3\mu \times 1\mu$, but filaments up to 20 μ long were found. The growth resulted in a curious opalescent change in the liquid above the whitish dense deposit, and it became markedly viscous so as to draw out into strings

Made up in Mayer's solution also, the same viscidity and gelatinous growth occurred, and this both in air and in hydrogen. We have since learnt that this syrup consists of dextrin, cane-sugar, and a little alcohol

Dextrin

The difficulty as to the nature of "Dextrin-Zucker" referred to above, led us to try dextrin itself, and owing to the kindness of Dr. Ruhremann some very pure material was to hand With Mayer's solution several experiments demonstrated that no obvious growth occurred, and it was clear that this dextrin is not the same for nutritive purposes as the "Dextrin Zucker" used previously.

Nor was dextrin made up with yeast-water of any use, though occasionally very slight indications of growth occurred during the first twenty-four hours, but no viscosity or sheathed bacteria formed

Malto-dextrin.

Owing to the kindness of Mr Ling, we were furnished with some prepared malto-dextrin, but neither as Mayer's solution, nor made up with yeast extract, was this sugar of any use as a medium for the growth of this bacterium

Maltose.

This sugar, made up as Mayer's solution, was found unsuited for the bacterium, either at high or low temperatures Nor was it more successful made up with yeast extract, a faint turbidity and flocculence occurred, but no trace of viscosity at 25° occurred in four days.

Milk-Sugar.

Milk-sugar, whether made up with yeast-water, Mayer's solution, asparagin, or peptone, proved quite useless as a food for the bacterium: no signs of growth appeared in the solutions at all, at high or low temperatures, in air, or in carbon dioxide. The organism was dormant only, however.

Soluble Starch.

It seemed worth while, in view of the gelatinous nature of vigorous growths, to see how the bacterium would behave towards soluble starch, but in no case could any evidence of growth be obtained in solutions containing 2 per cent. of soluble starch made up with Mayer's solution, or with yeast-water. The bacteria lay dormant only at the bottom, as experiments showed.

It is evident from the foregoing that of all the sugars tried, saccharose is the one which favours the growth of the bacterium, but even in saccharon the growth is distinctly favoured by the addition of yeast extract. Some experiments were consequently started to test the effect of the yeast extract

Raw Yeast-Water.

Fresh yeast, squeezed and drained, and then ground up with kieselguhr and extracted with water, was allowed to stand all night and filtered through porcelain Employed alone it was of no use as a medium for the growth of the bacterium, nor would the latter develop in this raw yeast-water, to which 5 per cent of cane-sugar was added. The latter fact was thought to be possibly due to the raw yeast-water having inverted the saccharose, and we have seen that glucose and levulose are not suitable media, and we explained similarly the failure of saccharon-Mayer's solution, to which this raw yeast-water was added, as well as failures with brown sugar, and with beet extract similarly made up That failure should follow with dextrin, with maltose, with levulose, milk-sugar, and with beer wort similarly made up, was only to be expected from experience with these media concocted with boiled yeast-water

Further experiments, however, led to the conviction that matters were more complicated than would be implied by this explanation

At one stage in the investigation, being impressed by the stimulus to growth afforded by the addition of (boiled) yeast-water to the saccharose solutions, we tried the effect of adding sugar to the raw yeast extract and sterilising by filtration through porcelain only, and were surprised to find that no growth whatever occurred at any temperature, *e.g.*, 18°, 23°, and 30° C This was afterwards explained as above—the yeast extract inverts the sugar before it has time to filter

We then tried a series of experiments as follows —Cultures at 17—20°, 25°, and 31° C, were made in 10 per cent. saccharon + 10 per cent yeast-water mixed raw and sterilised by filtration only; no traces of growth occurred in 10 days. The same failure was realised with 10 per cent saccharon alone sterilised by filtration only; with yeast extract only sterilised by filtration only; and with yeast extract sterilised by filtration and then boiled before adding to the filtered sterile 10 per cent. saccharon solution In no case did any sign of growth occur.

It is therefore clear that the filter either holds back some body necessary for the nutrition of the bacterium, or destroys it in its passage through the pores. Also that raw yeast extract in some way spoils the sugar (saccharose) as a food material, probably by inverting it. And nevertheless the bacterium flourishes in conjunction with the living yeast in saccharose solutions. Here is a puzzle which we have not succeeded in explaining.

It may be merely noted that numerous trials were made in other media than those mentioned, among which glycerine and yeast extract, alcohol with saccharon and yeast extract, starch treated with diastase, also potato, carrot, and milk are the most important. No growth of significance was obtained in any case, and the results may be neglected.

The Acidity of the Cultures.

Several tests showed that the cultures of the bacterium are acid, and the following experiments were made. Sterilised blocks of marble were placed in the culture tubes before they were steamed, and then the infections made as before. In saccharose yeast extract, the active growth which resulted was accompanied by a more diffused viscosity than before, and gas bubbles (CO_2) ascended for days. Tubes of mixed dextrose and levulose with yeast extract, treated exactly similarly, became turbid, and gave off bubbles, but no trace of viscosity resulted; the abundant flocculent deposit consisted entirely of the non-sheathed form of the bacterium.

As will appear later, the acid which causes this liberation of CO_2 is mainly acetic acid.

On the Nature of the Gelatinous Matter or Slime.

A number of experiments were made to determine the nature of the viscous slime and the jelly-like matrix holding the bacteria. Although reasons have already been given for concluding that this is really nothing more than the swollen cell-walls or sheaths investing the bacteria, the possibility of the opalescence and viscosity of the saccharose media being due to the direct action on the sugar of some enzyme or other body excreted by the organism requires investigation, for it is conceivable that such slimes might arise in any of three ways:

(1) As products of metabolism from the interior of the cells, such as certainly occur in the glands of higher plants, *e.g.*, the mucilage hairs of ferns.*

(2) As products of the action outside the cells of some enzyme-like body which escapes from the organism and acts directly on the sugar.†

(3) As products of conversion of the cell-walls of the organism, these swelling up and becoming diffluent as in the case of the ginger-beer plant.‡

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* See Gardiner and Ito, 'Annals of Botany,' vol. 1, p. 27.

† See Ritsert, 'Cent. f. Bakt.,' vol. 11, p. 830.

‡ 'Phil. Trans.,' B, 1892, *loc. cit.*

It seemed impossible to test the first suggestion on such minute cells, but attempts were made to test the second one by the following experiment :--

The bacterium was grown in saccharose yeast extract inside a porcelain filter plunged into the same solution ; the gelatinous matrix was formed in abundance *inside* the filter, but none was developed outside although the liquids communicated freely through the pores of the filter. Of course the reply may be made that an enzyme of this nature may be unable to traverse the fine pores, and the question must be regarded as still open.

These gelatinous sheaths or "capsules" are now known in many Schizomycetes, among the best examples being that of *B. vermiforme** and *Leuconostoc*,† and it is now pretty generally agreed that these sheaths are composed of dextran,‡ and Liesenberg and Zopf§ made a curious observation with reference to its formation ; they found that the addition of calcium chloride favoured the development of the sheath.

Calcium Chloride.

Zopf's paper suggested that we should test the effect of $CaCl_2$, and accordingly solutions of Liebig's extract, peptone-saccharon, and $CaCl_2$ were tried.

In the slightly alkaline liquid at 32° a mere shimmering turbidity was observed in twenty-four hours, and in four days a dense gelatinous clot formed below the turbid liquid.

At 23° the alkaline liquid showed similar turbidity on the second day, and had formed very little gelatinous deposit in four days. In a week, however, it resembled that at 32° C.

The same solution slightly acidulated with a drop of HCl was slightly more turbid in twenty-four hours, but very little of the jelly formed even in a week.

At the end of the week both set of tubes were distinctly acid, and evidently the formation of the jelly is favoured by the slight alkalinity of $CaCl_2$.

This seemed to strengthen the supposition that our bacterium may be the same as Van Tieghem's *Leuconostoc*, but a close comparison does not bear out this view.

It is nevertheless interesting to observe that *Leuconostoc* inverts saccharose, and only forms sheaths in presence of that sugar or of grape-sugar ; that these sheaths are explained as the swollen cell walls, and many other features exist in common with our form.

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* Marshall Ward, 'Phil. Trans.,' B, 1892, *loc. cit.*

† Van Tieghem, 'Ann. des Sci. Nat.,' 6th Series, Bot., vol. 4, 1878.

‡ Scheibler, 'Vereinzeitsch. f. Rubenzucker Ind.,' (1874), p. 24.

§ 'Beitr. z. Phys. u. Morph. niederer Organismen,' Heft 1, 1892, p. 1.

The differences may be important in various degrees. *Leuconostoc* appears smaller and shorter, and withstands high temperatures; it succeeds well in grape-sugar, its characters on gelatine media appear to be different, it can be cultivated in milk, and it forms lactic acid in sugar solutions.

How far the differences can be insisted upon cannot be determined until both organisms have been tested side by side.

There is one further observation to be made in support of our contention that the viscosity depends on the deliquescence of the swollen cell-walls. On agar media, which exude water, the growths are slimy rather than gelatinous, and the longer the gelatine cultures are kept, provided they are not allowed to evaporate, the more diffluent the gelatinous lumps become. In liquid cultures, moreover, the gelatinous clot does not spread evenly through the liquid, but remains around the motionless organism.

Mixed Cultures.

Several attempts were made to obtain the typical clumps of the compound organism by infecting tubes of yeast extract and saccharon and other media with both the bacterium and the yeast separately cultivated pure. The success was only partial, however, though it was not difficult to obtain a viscous clot at the bottom of the tubes in which both bacteria and yeast were embedded, the typical stiff jelly clumps floating in the liquid were not formed, and here again we must conclude that some definite proportion of each is necessary.

On the Chemical Changes incident to the Fermentations.

The jelly like masses of which the organism consisted set up a very vigorous fermentation in beer wort and in solutions of various sugars. Careful cultures showed, as already stated, that the jelly was composed essentially of a bacterium associated with a yeast, and a long series of our experiments has been directed towards ascertaining what part each played in the fermentation, and how far they assisted or impeded each other.

In the greater number of these experiments five flasks were used, each of about 250 c.c. capacity. 150 c.c. of the culture fluid were placed in each, and four of them were sown with pure cultures: (α) of the yeast alone, (β) of the bacterium alone, (γ) of the yeast and the bacterium separately, (δ) of the two in their ordinary condition of association, forming a lump of the jelly. The fourth flask contained only the culture-fluid with no organism of either kind.

The culture-fluids were usually a 10 per cent. solution of some particular sugar to which 10 per cent. of its volume of yeast-water had been added to furnish the necessary combined nitrogen for the growth of the organism. The yeast-water was prepared by boiling yeast in water for several minutes, and then filtering and sterilising. In one experiment an ordinary beer wort was used without addition of more nitrogenous matter.

The liquids were carefully and repeatedly sterilised in the flasks before the organisms were added.

The fermentations were conducted at a temperature of about 20° C. The marked feature of the fermentation set up by the conjoint organism was the production of a considerable acidity, the liquid after a few days having the appearance and flavour of lemonade. The acidity proved to be due to acetic and succinic acids.

As considerable differences of behaviour were soon manifested, we subjoin the results of typical fermentations.

	Alcohol Per cent	Acetic acid Per cent	Succinic acid Per cent
I. Beer wort, 150 c.c.—			
The yeast produced	3·5	0·0124	0·068
The bacterium produced	0	0·1734	0·3
II. Cane sugar, 150 c.c. of 10 per cent solution containing 15 c.c. yeast water:—			
Yeast produced	5·0	0·01	0·057
Bacterium produced	0	0·7	0·57
Yeast + bacterium (in the form of the conjoint organism) produced ..	5·0	0·018	0·078
III. Dark brown sugar (mixture of cane sugar and levulose), proportions as in II.—			
Yeast produced .	4·75	0·026	0·137
Bacterium produced . .	0	0·596	0·416
Yeast + bacterium separately sown produced . .	4·0	0·124	0·1
Conjoint organism produced	3·7	0·306	0·168
IV. Grape sugar, proportions as in II.—			
Yeast produced .	2·2	0·013	0·049
Bacterium produced .	0	0·012	0·018
Yeast + bacterium produced	2·0	0·02	0·097
Conjoint organism produced	2·0	0·15	0·046

The formation of the alcohol was thus shown to be due exclusively to the yeast, and in its production the influence of the bacterium was not manifested. The yeast was found to be capable of fermenting both glucose and fructose (levulose), but to be more active in the presence than the absence of the latter Only half the amount of alcohol was formed when the fructose was excluded The latter sugar was more favourable also to the acid fermentation by the yeast. If we compare Experiments II and III, we find that while the same quantity of alcohol was formed in both cases, the proportion of both acetic and succinic acids was about doubled in the presence of fructose

The bacterium, however, was responsible for the greater amount of the acid formation While it caused the production of both acetic and succinic acids in greater proportion than the yeast, it yielded far more relatively of the former. In the experiment with beer wort it produced fifteen times as much acetic acid as the yeast, it only gave rise to four to five times as much succinic. With cane-sugar solution it formed seventy times as much acetic and ten times as much succinic acid as the yeast The same result was obtained with cane-sugar and fructose

With grape-sugar the bacterium, like the yeast, could do but little. It produced about the same amount of acetic acid, but scarcely more than one-third as much succinic.

The experiments on the fermentations confirm the view expressed in the early portion of the paper, that cane-sugar is the most favourable medium for the bacterium, but before it is of use to it, it undergoes inversion

The association of the two organisms together introduced a somewhat curious feature of the fermentation The yeast continued to produce alcohol in the same proportion as when alone, but the acid fermentations were modified considerably Comparative experiments were made with cane-sugar, dark brown sugar containing fructose, and grape-sugar In the first two cases the amount of both acetic and succinic acids produced by the conjoint organism was distinctly less than that which was formed by the bacterium alone. In the grape-sugar fermentation the contrary was the case

The association of the two organisms into the jelly-like clumps was not without its effect upon the progress of the fermentation, and this effect again appeared in connection with the process of the acidification.

With the dark brown sugar the conjoint organism produced twice as much acetic acid as the two separate constituents working together, but only one and a half times as much succinic. With grape-sugar there was a diminution of the succinic acid, which fell to one-half the quantity produced by the yeast and the bacterium, while not in such close association. The acetic acid, on the other hand, increased. In the first two cases again, the presence of the yeast was very considerably inhibitory of the activity of the bacterium, the latter producing far more acid of both kinds when it was alone in the fermenting liquid.

One or two features of the fermentations call for comment. The progress of the fermentation in an acid liquid was so great as to suggest that an enzyme was excreted by the organism, but careful search proved that this was not the case Both the conjoint organism and its two constituents are normally aerobic, but in all cases the fermentations will proceed, though with slightly less vigour, in an atmosphere of CO_2 The decomposition effected by the bacterium is accompanied by only a very slow evolution of this gas, not greater indeed than would be due to its respiration In one experiment which was carried on for two months, there was an output of 0 005 gram of CO_2 per day, the gas being absorbed by caustic potash as it was given off, and the fermentation being consequently aerobic

The intimate association of the two organisms in the lumps of jelly suggested a symbiotic relationship Experiments made to ascertain how the two affected each other failed however to bear out this view The influence of the bacterium we have seen to be largely shown in the formation of relatively considerable quantities of both acetic and succinic acids Cultivations of the yeast were consequently made in the presence of different proportions of acid, with the expectation of finding that an acid medium would be advantageous to it

Three flasks were prepared, each containing 90 c c of 10 per cent solution of cane-sugar to which 10 c c of yeast-water were added. These were then carefully sterilised To flask A, acetic acid was added till 0 1 per cent was present, B contained 0 25 per cent., and C 0 5 per cent. of the same acid Each was then infected with 1 c c of a pure culture of the yeast, and they were allowed to ferment at the laboratory temperature (about 10° C) A control was prepared in a fourth flask, no acid being added. The fermentation which resulted in A was about equal to that in the control, that in B was less vigorous, and that in C was very feeble After two days, while the fermentation was still active, they were all filtered on to tared filters, and the quantity of yeast weighed. The control was then found to contain 0·09 gram of yeast, while the other flasks contained respectively—A, 0 106, B, 0·079, and C, 0 027 gram. So far as the growth of the yeast was concerned, 0 1 per cent of acetic acid was favourable, but 0 25 per cent. was slightly inhibitory, while 0 5 per cent. was markedly so

The alcohol was then distilled off and estimated A contained 2 per cent., B 1 33 per cent, and C 1 per cent., while the control contained 1·66 per cent. of the spirit. These figures agree with the conclusion based upon the weight of the yeast. The same results were obtained when lactic acid was substituted for acetic.

As in the original experiments made with cane-sugar, the bacterium produced more acetic acid than the highest proportion used in these fermentations, it is clear that the bacterium does not conduce in this way to any increase of growth or activity of the yeast.

It seemed possible that the bacterium might assist the latter by fixing nitrogen from the air, as bacteria have been found to do in other cases of symbiosis Careful experiments showed, however, that neither the conjoint organism nor the bacterium alone could grow in a culture fluid which did not contain combined nitrogen. A quantitative estimation of nitrogen was made of two fermentations of cane-sugar, one by yeast alone, the other by the conjoint organism, a control of the culture fluid alone being examined simultaneously The nitrogen was determined by Kjeldahl's process The initial amount of nitrogen present was 0 013 gram Neither flask showed any variation from this quantity at the end of the fermentation

The presence of the bacterium was seen consequently to be of no service to the yeast, but on the other hand to be disadvantageous to its growth

The yeast was, on the other hand, found to be of some value to the bacterium During the fermentations the former was found to excrete a certain amount of various extractives into the liquid, which had a distinctly nutritive value to the latter The bacterium grew very much better at the expense of these extractives than it did when supplied with combined nitrogen in the form of ammonium tartrate, or of asparagin Comparative experiments made with the extractives prepared from a fermentation showed that neither asparagin nor an ammonium salt could minister to its development so readily as they did. This view was also supported by the fact that yeast-water had already been found to be the best form of supplying combined nitrogen artificially to cultures of the isolated bacterium

The alcohol was of no nutritive value to the latter When it was cultivated in the presence of varying quantities of the spirit, it made no use of it, and the original quantity of alcohol was found to be present in the liquid at the conclusion of the experiment

The relationship between the two is not therefore one of symbiosis The bacterium appears to be a saprophyte, thriving at the expense of the nitrogenous excreta of the yeast It is not at all parasitic on its neighbour, the yeast never being injured by its presence until sufficient acid has been produced to cause a secondary inhibition

In the formation of the acetic acid this bacterium is peculiar It cannot be referred to the ordinary group of acetifying organisms,* as it has not the power of affecting alcohol as they do Its action on the sugar seems to be a direct one, causing a formation of acid at the expense of the latter, without setting up any preliminary fermentation. *Bacterium aceti* (Brown) has also this power in the presence of oxygen, but it can oxidise alcohol in addition.

The immediate antecedent of the acid appears to be fructose.

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* See Beijerinck, 'Cent f Bakt,' vol. 4, 1898, for a summary of the known acetic organisms

Though the bacterium flourishes in solutions of cane-sugar, it does not convert the latter immediately into the acids of the fermentation, but hydrolysis takes place first. In one experiment 150 c.c. of a 10 per cent. solution of cane-sugar, containing 15 c.c. of yeast water, were infected in a sterile flask with a pure culture of the bacterium, and fermentation was allowed to proceed for twelve days at the temperature of the laboratory. At the end of that time the acetic acid was distilled off, and the residue poured into alcohol to precipitate another constituent, which will be referred to below. The remainder of the sugar was left in solution in the alcohol. After filtration the alcoholic liquid was evaporated to a syrupy consistency, and the residue taken up with water. It was then divided into two equal portions, and one of them was acidified with sulphuric acid till the concentration of the latter was 2 per cent. It was then boiled on a water-bath for two hours and carefully neutralised. An aliquot part of each was then titrated with Fehling's solution, and the cupric oxide formed was weighed. The weights of the two were almost identical, differing by only 0.002 gram. The inversion of the cane-sugar by the organism had consequently been practically complete.

A few bacteria only have been found to secrete invertase. Fermi and Montesano found that *Bacillus megatherium*, *B. fluorescens liquefaciens*, the red Kiel bacillus, and *Proteus vulgaris* were capable of producing it in bouillon to which cane-sugar had been added. Van Tieghem showed that it was secreted by *Leuconostoc mesenterioides*. To these the bacterium under observation must now be added. The following experiment shows that the inversion noticed in the last case was due to an excreted enzyme.

A good culture was made in cane-sugar solution, and, after a few days, was filtered under pressure through a Chamberlandt's porcelain tube. Three flasks were taken, and 15 c.c. of cane sugar solution of 2 per cent. concentration placed in each. They were then carefully sterilised. To A and B, 2 c.c. of the filtrate from the culture were added, the filtrate having been found free from organisms by careful microscopic examination; B was then boiled and allowed to cool slowly. They were all kept for a few days at the temperature of 22° C. in an incubator. On titration with Fehling's solution, C, the control, gave no reduction, B a slight one, and A threw down a considerable precipitate of cuprous oxide. The small reduction in B was due no doubt to a trace of invert sugar present in the culture fluid added to the flask. The much greater reduction in A was due to invertase which the bacteria had excreted.

The experiments quoted in the early part of this paper show, however, that the inversion must take place gradually and the fructose be supplied almost as it is wanted. In presence of fructose only, growth is almost impossible.

Attention has been called in the first part of this paper to the amount of viscous material which the bacterium produces in solutions of cane sugar There is a great similarity between its action in this respect and that of Van Tieghem's *Leuconostoc*

An examination was made of the viscous material, a special culture being made in a large flask for the purpose The slimy material was found to be slowly soluble in water, yielding an opalescent solution. When poured into an excess of alcohol, it gave a bulky flocculent precipitate This was allowed to settle and the alcohol was decanted, and the wet precipitate thrown on to a filter When all the spirit had drained away, it was taken from the filter and stirred with water in a beaker A considerable quantity dissolved, but a good deal of residue was left in suspension The watery solution was filtered off and examined

It gave a reddish-purple colour on the addition of iodine, but had no action on Fehling's fluid Heated with 2 per cent of sulphuric acid on a water-bath for two hours and then neutralised, it gave evidence of the presence of sugar It reduced Fehling's fluid on boiling, and yielded an osazone when treated with phenylhydrazine acetate It deflected a ray of polarised light, and had a specific rotatory power of $[\alpha]_D = +130$

The residue, which was insoluble in water, was coloured violet on the addition of iodine It had no action on Fehling's solution, but was converted into a reducing sugar by boiling it with 2 per cent of sulphuric acid It was readily soluble in a 10 per cent solution of caustic soda, and less freely in a 1 per cent solution Neutralisation or dilution did not cause it to be reprecipitated The solution had no action on polarised light

There were thus found to be present in the viscous liquids two distinct carbohydrates, which possessed much in common with Scheibler's *dextran*, but which were not quite identical with the latter. They both appeared to be members of the hemi-celluloses

The exact relation of these bodies to the bacterium has not been determined. While there are some grounds for thinking that they are not so much products of fermentation in the strict sense as of its ordinary biological processes, being perhaps only the substance of the different sheaths to which allusion has been made in the first part of this paper, it is not certain that they may not be regarded as products produced altogether outside the organism, in consequence of an alteration of the fructose produced by the hydrolysis It is altogether unlikely that they resulted from the glucose moiety of the inversion, as this sugar is not a favourable medium for the growth of the organism, and such cultivations as are possible in solutions of glucose do not show the presence of any viscous material

"Experiments in Micro-metallurgy.—Effects of Strain. Preliminary Notice." By James A. Ewing, F.R.S., and Walter Rosenhain, 1851 Exhibition Research Scholar, Melbourne University. Received and Read March 16, 1899

[Plates 1—5.]

Much information has been obtained regarding the structure of metals by the methods of microscopic examination initiated by Sorby and successfully pursued by Andrews, Arnold, Charpy, Martens, Osmond, Roberts-Austen, Stead, and others. When a highly polished surface of metal is lightly etched and examined under the microscope, it reveals a structure which shows that the metal is made up in general of irregularly shaped grains with well defined bounding surfaces. The exposed face of each grain has been found to consist of a multitude of crystal facets with a definite orientation. Seen under oblique illumination, these facets exhibit themselves by reflecting the light in a uniform manner over each single grain, but in very various manners over different grains, and, by changing the angle of incidence of the light, one or another grain is made to flash out comparatively brightly over its whole exposed surface, while others become dark.

It is also well known that the grains are deformed when the metal is subjected to such processes as cold hammering, or cold rolling, or wire-drawing. On polishing and etching a piece strained in any such way, the grains are found to be on the whole longer in the direction in which the metal is extended than in other directions. But on heating the metal sufficiently a re-formation of structure occurs, and the grains are found to have again assumed forms in which there is no direction of predominating length. In iron this recrystallisation occurs at a red heat. It is also known that prolonged exposure of iron to a temperature of about 700° C. tends to produce a larger granular structure than is found if the metal is somewhat quickly cooled from a higher temperature.

The grains appear to be produced by crystallisation proceeding, more or less simultaneously, from as many centres or nuclei as there are grains, and the irregular more or less polygonal boundaries which are seen on a polished and etched surface result from the meeting of these crystal growths. The grains are, in fact, crystals, except that each of their bounding surfaces is casually determined by the meeting of one growth with another. This is, we believe, the view usually accepted by metallurgists;* but there is considerable difference of opinion as to the part played by foreign matter in possibly contributing to form a cement at the intergranular junctions.

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* See especially two papers by Mr. J. E. Stead ('Jour. Iron and Steel Inst.,'

The experiments, of which this is a preliminary account, have been directed to examine the behaviour of the crystalline grains when the metal is subjected to strain

For this purpose we have watched a polished surface under the microscope while the metal was gradually extended until it broke. By arranging a small straining machine on the stage of the microscope, we have been able to keep under continuous observation a particular group of crystalline grains while the piece was being stretched, and have obtained series of photographs showing the same group at various stages in the process Strips of annealed sheet iron, sheet copper, and other metals have been examined in this way We have also observed the effects of strain on the polished surfaces of bars in a 50-ton testing machine by means of a microscope hung from the bar itself, and have further observed the effects of compression and of torsion

When a piece of iron or other metal exhibiting the usual granular structure is stretched beyond its elastic limit a remarkable change occurs in the appearance of the polished and etched surface, as seen by the usual method of "vertical" illumination A number of sharp black lines appear on the faces of the crystalline grains at first they appear on a few grains only, and as the straining is continued they appear on more and more grains On each grain they are more or less straight and parallel, but their directions are different on different grains. At first, just as the yield-point of the material is passed, the few lines which can be seen are for the most part transverse to the direction of the pull As the stretch becomes greater oblique systems of lines on other grains come into view

The photograph, fig 1 (Plate 1), taken from a strip of transformer plate (rolled from Swedish iron and annealed after rolling), gives a characteristic view of these lines as they appear after a moderate amount of permanent stretching, but long before the iron has reached its breaking limit.

The appearance of each grain is so like that of a crevassed glacier, that these dark lines might readily be taken for cracks. Against this, however, was the consideration that an over-strained piece of iron recovers its original elasticity after a period of rest, though, as we found, the dark lines did not disappear when recovery took place, and further that sharp lines of the same nature were not seen on the surface of metal which was polished and etched after straining.

The real character of the lines is apparent when the crystalline constitution of each grain is considered. They are not cracks, but *slips* along planes of cleavage or gliding planes.

Fig. 2 is intended to represent a section through the upper part of two contiguous surface grains, having cleavage or gliding planes as indicated by the cross-hatching, AB being a portion of the polished surface. When the metal is pulled beyond its elastic limit, in the direction of the arrows, yielding takes place by finite amounts of slips occurring at a limited number of places in the manner shown at *a*, *b*, *c*, *d*, *e* (fig. 3). This slip exposes short portions of inclined surfaces, and when viewed under normally incident light, these surfaces appear black because they return no light to the microscope They are consequently seen as dark lines or narrow bands, extending over the polished surface in directions which depend on the intersection of the polished surface with the surfaces of slip

We have proved the correctness of this view by examining these bands under oblique light When the light is incident at only a small angle to the polished surface, the surface appears for the most part dark, but here and there a system of the parallel bands shines out brilliantly, in consequence of the short cleavage or gliding surfaces which constitute the bands having the proper inclination for reflecting the light into the microscope Fig 4 is the photograph of a strained piece of Swedish iron illuminated in this way. The magnification is 280 diameters. The groups of parallel bright bands which appear in the photograph may readily be observed under the microscope to be exactly coincident with the black bands seen under vertical illumination, and by changing the angle of incidence of the oblique light, the same bands may be made to appear dark on a faintly luminous ground. Rotation of the stage to which the strained specimen is fixed makes the bands on one or another of the grains flash out successively, with kaleidoscopic effect. In what follows we shall speak of these lines as slip bands. Fig. 1, through a mixed illumination, shows some of the slip bands bright and some dark.

Incidentally fig 4 illustrates the fact that oblique lighting picks out the boundaries of the crystalline grains, showing that these boundaries are marked by inclined surfaces connecting grains whose faces are at different levels. This is observed also in the etched surface of the metal before straining. The boundaries, which appear dark under vertical light, are bright on one side of each crystalline grain, when the light falls with grazing incidence from one side But the sloping surfaces which mark the boundaries between the grains have by no means the sharply definite inclination which characterises the surfaces which form the slip bands. One or more groups of slip bands will shine out very brightly when the light has a particular angle of incidence, and will vanish when the incidence is slightly changed. The boundaries are not in general so bright, but they remain fairly bright while the incidence is changed through wide limits.

When the metal is much strained a second system of bands appears on some of the grains, crossing the first system at an angle, and in some cases showing little steps where the lines cross These bands are clearly due to slips occurring in a second set of cleavage or gliding surfaces. An example of the crossed systems of bands will be seen in fig. 8. The crystals in metals are generally cubical, but the angle at which the intersecting systems of bands cross depends on the inclination of the polished surface to the planes of cleavage. Occasionally a third system of bands may be seen

As straining proceeds the originally smooth surface of the specimen becomes roughened by the surface grains changing in their relative levels and also becoming more or less inclined, as well as more or less stepped All this happens in consequence of the slips which they and their neighbours undergo To this is due the dull appearance which an originally bright surface assumes when the metal is overstrained Under the microscope the strained surface is seen to be full of ups and downs, and a continuous alteration in focus is required to trace the system of bands, even over the face of a single grain

When the experiment is made with a polished but unetched specimen the slip bands appear equally well The boundaries of the grains are invisible before straining, but they can be distinguished as the strain proceeds, for the slip bands form a cross-hatching which serves to mark out the surface of each grain. To strain a polished but unetched specimen reveals in a striking way the granular character of the structure *

Figs. 5, 6, and 7 are selected from a series of photographs showing under a magnification of 140 diameters, the same group of crystalline grains in a specimen of soft wrought iron at various stages of straining by pull The arrows show the direction in which the pull was applied

Fig 5 shows the group before straining began Fig 6 is the same group after the strain had been carried some way past the yield point Fig 7 is the same group after the piece had suffered considerable further extension in the same direction Comparison of the three will show how the grains change their shape in consequence of the slips which occur in them, and also how the faces of the grains become tilted and altered in relative level.

Fig 8 is another sample of iron strained by pull The specimen in this case was a bar of Swedish iron, in which a comparatively large crystalline structure had been developed by annealing for some hours at 700° C The photograph was taken after the bar had been broken in the testing machine, and shows with a magnification of 400 diameters a portion of the surface not far from the place of fracture The large grain which appears to the left in fig. 8 measured 0 16 mm. in the direction of the arrows before straining, and was extended by the strain in that direction to about 0 2 mm The slip bands upon it are on the average 1/400 mm. apart. This applies to both of the two systems of bands which appear in the photograph. The apparent

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* The fact that the crystalline structure is revealed when a specimen with a polished unetched surface is strained has already been pointed out by Charpy ('Comptes Rendus,' vol 123, 1896, p 225).

width of the slip bands themselves is too small to be measured with any accuracy, it does not appear at any place here to exceed 1/2000 mm

The slip-bands are developed by compression as well as by extension. Fig 9 is a photograph (at 400 diameters) from the polished and etched side of a block of Low Moor iron compressed in the testing machine sufficiently to give it a considerable amount of permanent set. The bands developed by compression have apparently all the characteristics which they present in stretched pieces, and we could not, by microscopic examination of the surface, distinguish, in this respect between the effects of compression and extension The irregular dark patches in fig 9 are streaks of slag

By twisting an iron bar well beyond the elastic limit the slip-bands are made to appear, for the most part in directions parallel and perpendicular to the axis of twist.

A strip of sheet metal, such as iron or copper, in the soft state, when bent and unbent in the fingers shows them well developed by the extension and compression of the surface

We have developed the slip-bands in iron, steel, copper, silver, lead, bismuth, tin, gun-metal, and brass In silver they show particularly well, the crystalline structure being large and the lines straight In copper also the lines are straighter and more regularly spaced than is general in iron Most of these metals have been tested in the form of blocks under compression A beautiful development of slip bands may readily be produced by pinching a button of polished silver or copper in a vice

In carbon steels we have found the slip-bands considerably more difficult to observe than in wrought iron The smaller granular structure of steel apparently makes the slip-bands correspondingly minute. In mild steel they are seen readily enough, but in a rather high carbon steel we succeeded in seeing them only with difficulty in the "ferrite" areas under a magnification of 1,000 diameters. A cast piece of the nearly pure iron used for dynamo magnets showed a relatively very large granular structure and well marked slip-bands

These experiments throw what appears to us to be new light on the character of plastic strain in metals and other irregular crystalline aggregates. Plasticity is due to slip on the part of the crystals along cleavage or gliding surfaces. Each crystalline grain is deformed by numerous internal slips occurring at intervals throughout its mass In general these slips no doubt occur in three planes, or possibly more, and the combination of the three allows the grain to accommodate itself to its envelope of neighbouring grains as the strain proceeds. The action is discontinuous it is not a homogeneous shear but a series of finite slips, the portion of the crystal between one slip and the next behaving like a rigid solid. The process of slipping is one which takes time, and in this respect the aggregate effect is not easily distinguishable from the deformation of a viscous liquid

We infer from the experiments that "flow" or non-elastic deformation in metals occurs through slip within each crystalline grain of portions of the crystal on one another along surfaces of cleavage or gliding surfaces There is no need to suppose the portions which slip to be other than perfectly elastic. The slip, when it occurs, involves the expenditure of work in an irreversible manner.

It is because the metal is an aggregate of irregular crystals that it is plastic as a whole, and is able to be deformed in any manner as a result of the slips occurring in individual crystals Plasticity requires that each portion should be able to change its shape and its position Each crystalline grain changes its shape through slips occurring within itself, and its position through slips occurring in other grains *

From what is known about the break down in elasticity which occurs as the immediate effect of overstrain and the subsequent recovery of elasticity after a period of rest, it would seem that the surfaces over which slip has occurred are at first weak, but heal with the lapse of time To discuss these points, however, lies beyond the scope of a preliminary notice

The experiments were made in the engineering laboratory at Cambridge, and are being continued We wish to take this opportunity of thanking Sir W Roberts-Austen and Mr. T Andrews for the great kindness with which, at the outset of our work, they gave us the benefit of their experience in preparing and observing microscopic specimens of metals

[*Note added April* 14, 1899 —In a specimen of cast nickel, which showed after etching a crystalline structure much resembling that of iron, but on a considerably smaller scale, straining developed minute slip-bands, which are clearly apparent under a power of 1,000 diameters.

We have also examined a specimen of pure gold by compressing a cast button with a polished face, not etched The straining reveals crystalline structure on a large scale, and in each of the crystalline grains there is a superb development of slip bands. They are long, nearly straight, exceedingly numerous, and very closely spaced. A power of 1,000 diameters is required to see them well. Two intersecting systems are common, and three are well seen in some of the grains, forming a regular geometrical network. The intergranular boundaries are sharply defined by the meeting of the slip-bands on each grain with those on its neighbours. The slip-bands in adjacent crystals meet in a way which demonstrates the absence of any appreciable quantity of foreign matter in the intergranular junctions.]

* Attention should be called in this connection to the experiments of Messrs. McConnel and Kidd on the plasticity of glacier ice ('Roy Soc. Proc,' vol 44, p 331). They found that bars cut from glacier ice, which is an aggregate of irregular crystals, are plastic

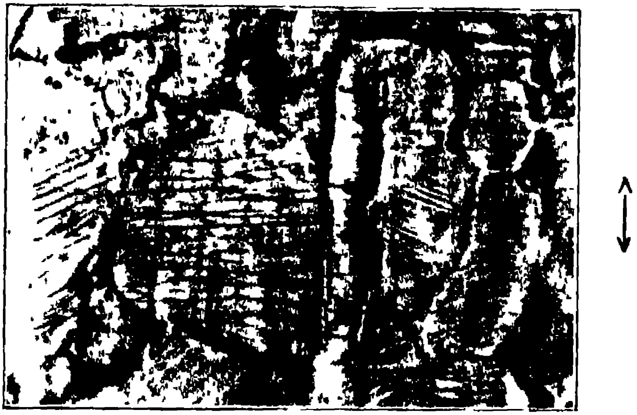

FIG. 1.—Soft Sheet Iron strained by tension. 400 diameters.

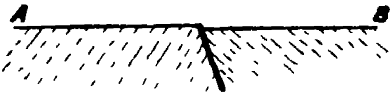

Fig 2 Before straining

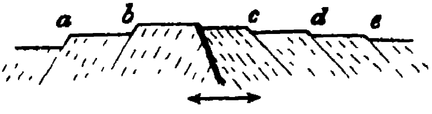

Fig 3 After straining

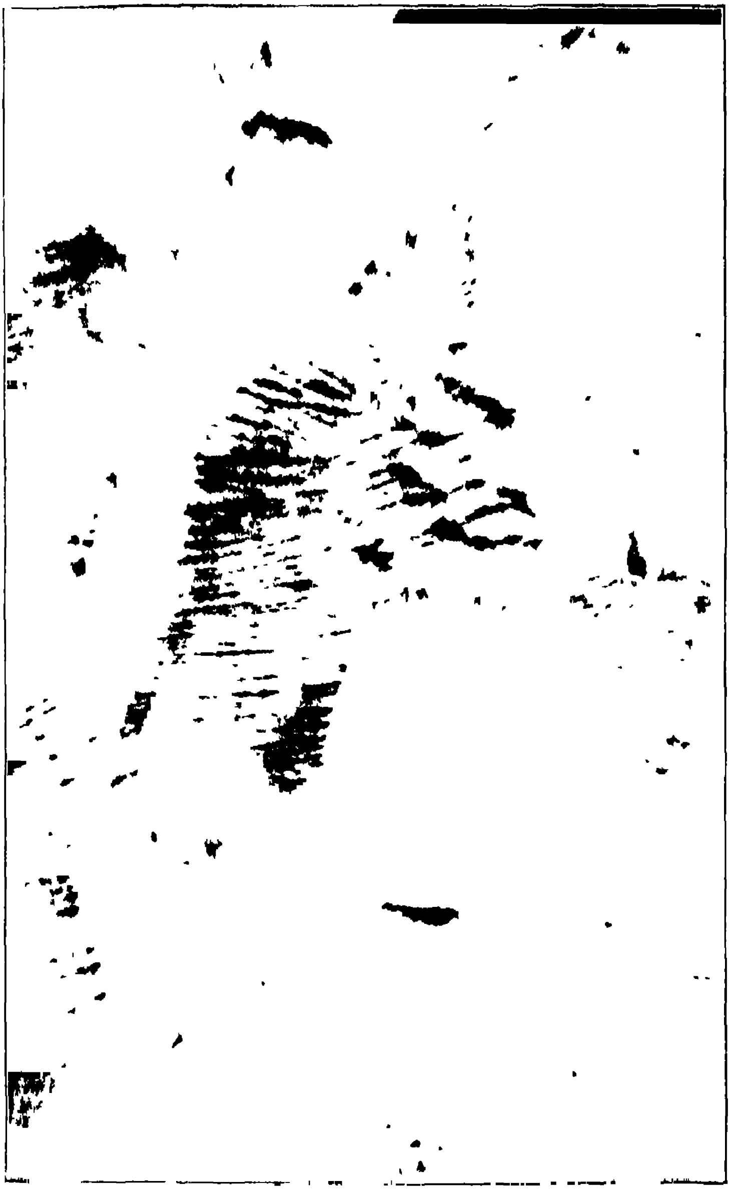

FIG. 4.—Swedish Iron much strained seen under oblique illumination 250 diameters

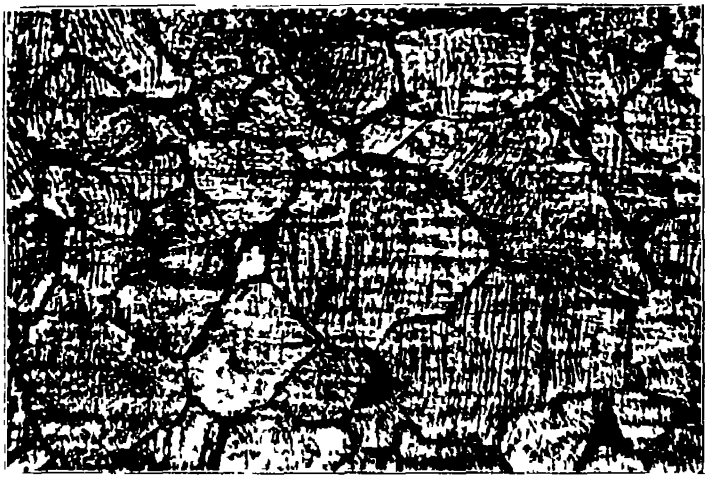

Fig. 5.—Before straining. 140 diameters.

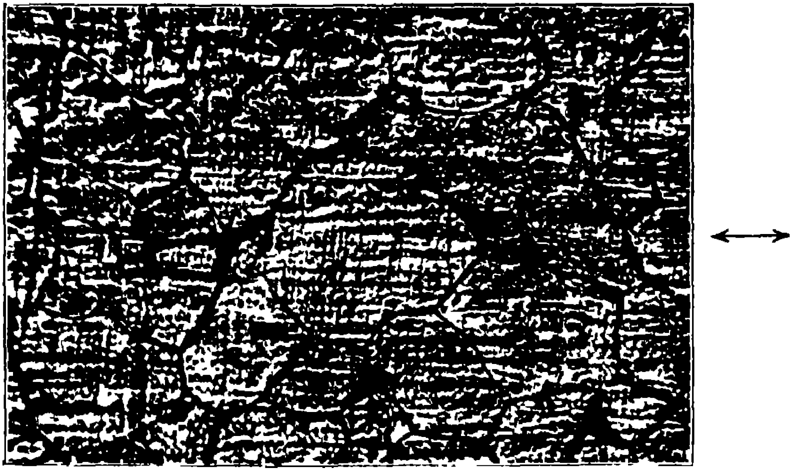

Fig. 6.—After moderate straining. 140 diameters.

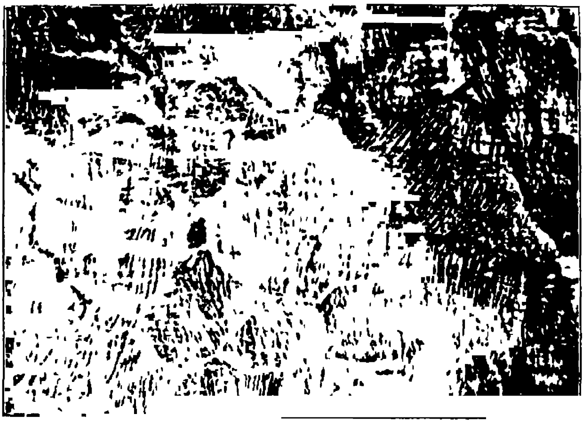

Fig. 7.—After further straining. 140 diameters.

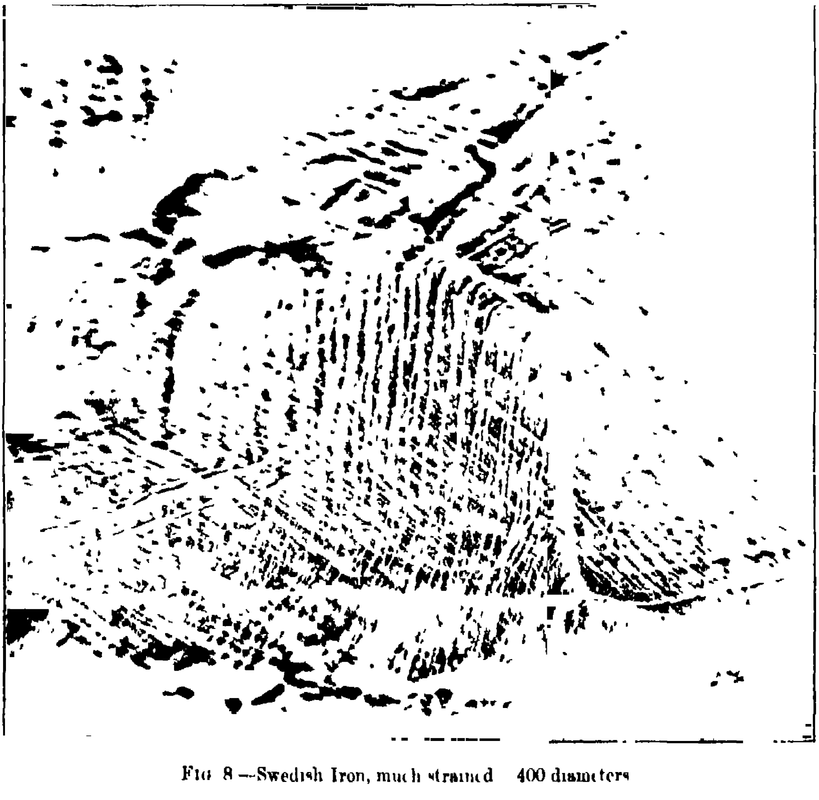

Fig. 8.—Swedish Iron, much strained. 400 diameters.

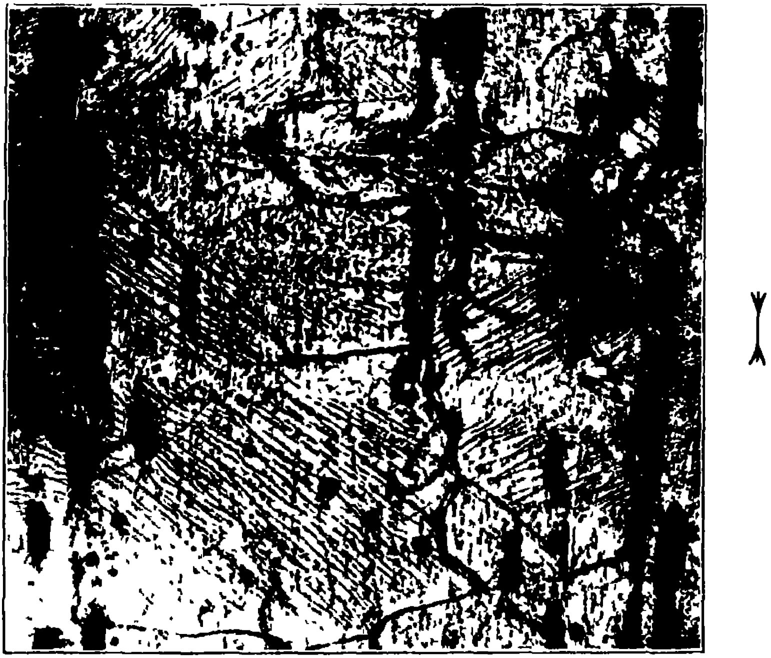

FIG. 9.—Low Moor Wrought Iron after compression. 400 diameters.

"The Physiological Action of Choline and Neurine" By F W. MOTT, M D, F R.S, and W. D HALLIBURTON, M.D, F R S.

Received March 13,—Read April 20, 1899.

(Abstract)

The cerebro-spinal fluid removed from cases of brain atrophy, particularly from cases of General Paralysis of the Insane, produces when injected into the circulation of anæsthetised animals (dogs, cats, rabbits), a fall of arterial blood pressure, with little or no effect on respiration. This pathological fluid is richer in proteid matter than the normal fluid, and among the proteids, nucleo-proteid is present. The fall of blood pressure, is, however, due not to proteid, nor to inorganic constituents, but to an organic substance, which is soluble in alcohol This substance is precipitable by phospho-tungstic acid, and by chemical methods was identified as choline. The crystals of the platinum double salt, which, when crystallised from 15 per cent. alcohol, are characteristic octahedra, form the most convenient test for the separation and identification of this base.

The nucleo-proteid and choline doubtless originate from the disintegration of the brain tissue, and their presence indicates that possibly some of the symptoms of General Paralysis may be due to auto-intoxication; these substances pass into the blood, for the cerebro-spinal fluid functions as the lymph of the central nervous system We have identified choline in the blood removed by venesection from these patients during the convulsive seizures which form a prominent symptom in the disease.

Normal cerebro-spinal fluid does not contain nucleo-proteid or choline, or if those substances are present, their amount is so small that they cannot be identified Normal cerebro-spinal fluid produces no effect on arterial pressure; neither does the alcoholic extract of normal blood or of ordinary dropsical effusions.

The presence of choline in the pathological fluids will not explain the symptoms of General Paralysis; for instance, it will not account for the fits just referred to. Its presence, however, is an indication that an acute disintegration of the cerebral tissues has occurred. If other poisonous substances are also present, they have still to be discovered.

Our proof that the toxic material we have specially worked with is choline, rests not only on chemical tests, but also on the evidence afforded by physiological experiments, the action of the cerebro-spinal substance exactly resembles that of choline. Neurine, an alkaloid closely related to choline, is not present in the fluid; its toxic action is much more powerful, and its effects differ considerably from those of choline.

Physiological Action of Choline.

The doses employed were from 1 to 10 c.c. of a 0·2 per cent. solution, either of choline or of its hydrochloride. These were injected intravenously.

The fall of blood pressure is in some measure due to its action on the heart, but is mainly produced by dilatation of the peripheral vessels, especially in the intestinal area This was dmonstrated by the use of an intestinal oncometer. The limbs and kidneys are somewhat lessened in volume, this appears to be a passive effect, secondary to the fall in general blood pressure The drug causes a marked contraction of the spleen, followed by an exaggeration of the normal curves, due to the alternate systole and diastole of that organ

The action on the splanchnic vessels is due to the direct action of the base on the neuro-muscular mechanism of the blood vessels themselves; for after the influence of the central nervous system has been removed by section of the spinal cord, or of the splanchnic nerves, choline still causes the typical fall of blood pressure The action of peripheral ganglia was in other experiments excluded by poisoning the animal previously with nicotine

Section of the vagi produces no effect on the results of injecting choline

We have obtained no evidence of any direct action of the base on the cerebral vessels.

Choline has little or no action on nerve trunks, as tested by their electrical response to stimulation This aspect of the subject has been taken up by Dr Waller and Miss Sowton, who will publish their results fully in a separate paper.

Choline has no effect on respiration.

The effect of choline soon passes off, and the blood pressure returns to its previous level This is due partly to the great dilution of the substance injected by the whole volume of the blood, and may be partly due to the excretion of the alkaloid, or to its being broken up into simpler substances by metabolic processes We could not find it in the urine.

If the animal has been previously anæsthetised with a mixture of morphine and atropine, the effect produced by choline is a rise of arterial pressure, accompanied by a rise of the lever of the intestinal oncometer Other anæsthetics cause no change in the usual results. We consider this observation of some importance, for it shows how the action of one poison may be modified by the presence of another. This has some bearing on General Paralysis, for the arterial tension in that disease is usually high, not low, as it would be if choline were the only toxic agent at work.

Physiological Action of Neurine

The doses employed varied from 1 to 5 c c. of a 0 1 per cent solution. These were injected intravenously.

Neurine produces a fall of arterial pressure, followed by a marked rise, and a subsequent fall to the normal level. Sometimes, especially with small doses, the preliminary fall may be absent. Sometimes, especially with large doses, by which presumably the heart is more profoundly affected, the rise is absent.

The effect of neurine on the heart of both frog and mammal is much more marked than is the case with choline , in the case of both choline and neurine, the action on the frog's heart is antagonised by atropine.

The slowing and weakening of the heart appear to account for the preliminary fall of blood pressure , in some cases this is apparently combined with a direct dilating influence on the peripheral vessels.

The rise of blood pressure which occurs after the fall, is due to the constriction of the peripheral vessels, evidence of which we have obtained by the use of oncometers for intestine, spleen, and kidney.

After the influence of the central nervous system has been removed by section of the spinal cord, or of the splanchnic nerves, neurine still produces its typical effects.

After, however, the action of peripheral ganglia has been cut off by the use of nicotine, neurine produces only a fall of blood pressure. It therefore appears that the constriction of the vessels is due to the action of the drug on the ganglia , in this, it would agree with nicotine, conine, and piperidine.

Section of the vagi produces no influence on the results of injecting neurine.

In animals anæsthetised with morphine and atropine, injection of neurine causes only a rise of blood pressure, which is accompanied with constriction of peripheral vessels.

Neurine produces no direct results, so far as we could ascertain, on the cerebral blood vessels

Neurine is intensely toxic to nerve-trunks (Dr. Waller and Miss Sowton).

It produces a marked effect on the respiration This is first greatly increased, but with each successive dose the effect is less, and ultimately the respiration becomes weaker, and ceases altogether. The animal can still be kept alive by artificial respiration.

The exacerbation of respiratory movements will not account for the rise of arterial pressure ; the two events are usually not synchronous, and an intense rise of arterial pressure (due, as previously stated, to contraction of peripheral blood vessels) may occur when there is little or no increase of respiratory activity or during artificial respiration.

As confirmatory of Cervello's statement that neurine acts like curare on the nerve endings of voluntary muscle, and to which he attributes the cessation of respiration, we may mention that after an animal has been poisoned with neurine, asphyxiation causes little or none of the usual convulsions

The full paper contains references to previous work on the subject, and complete details of the methods used, and the cases investigated; it is illustrated by reproductions of numerous tracings.

[*Note added April* 20, 1899 —It should be mentioned that in the cases of brain atrophy referred to, the cerebro-spinal fluid was removed soon after death. Since the foregoing abstract was written, we have, however, had the opportunity of examining two specimens removed during life by lumbar puncture, and the results of our experiments with these corroborate the conclusions previously arrived at.]

"On Intestinal Absorption, especially on the Absorption of Serum, Peptone, and Glucose" By E WAYMOUTH REID, F.R S., Professor of Physiology in University College, Dundee St Andrew's University, N.B Received March 30,—Read April 20, 1899.

(Abstract)

The experiments detailed in the full paper deal with the absorption from the intestine of the animal's own serum, and of solutions of glucose and peptone. The method employed has been that introduced by Leubuscher, in which two loops of intestine are simultaneously employed, the one the experimental, and the other the control, loop.

The conclusions arrived at are as follows ·—

1. A physiological activity of the intestinal epithelium in the act of absorption is demonstrated by—

(*a*) The absorption by an animal of its own serum (or even plasma) under conditions in which filtration into blood capillaries or lacteals, osmosis, and adsorption are excluded.

(*b*) By the cessation or diminution of the absorption of serum when the epithelium is removed, injured, or poisoned, in spite of the fact that removal, at any rate, must increase the facilities for osmosis and filtration

2. The activity of the cells is characterised by a slower uptake of the organic solids of the serum than of the water, and a rather quicker uptake of the salts than of the water. The relations to one another of the absorptions of these various constituents is variable in different regions of the intestinal canal (upper ileum, lower ileum, and colon).

3. No evidence can be obtained of specific absorptive fibres in the mesenteric nerves.

4. The state of nutrition of the cells is the main factor in their activity, and this is intimately associated with the blood supply.

5. In reduction of the rate of absorption, without detachment of epithelium, the absorption of the various constituents of serum is reduced in the proportion in which they exist in the original fluid.

6 The activity of the cells may be raised by stimulation with weak alcohol, without evidence of concomitant increase of blood supply

7. The bile has no stimulant action on the cells

8. The cells exhibit an orienting action upon salts in solution (sodic chloride especially) In a loop of gut with injured cells sodic chloride enters the lumen from the blood, at a time when it is being actively absorbed from a normal control loop in the same animal (This fact was first noted by O Cohnheim)

9. The absorption of water from solutions introduced into the gut is dependent upon two factors :—

(*a*) The physical relation of the osmotic pressure of the solution in the gut to the osmotic pressure of the blood plasma.

(*b*) The physiological regulation of the difference of osmotic pressure by the orienting mechanism of the cells

10. The chief factor in the absorption of peptone is an assimilation (or adsorption) by the cells, while in the absorption of glucose, diffusion, variable by the permeability of the cells (and so, probably, related to their physiological condition) is the main factor

11. By removal of the epithelium, the normal ratio of peptone to glucose absorption is upset, and the value tends to approach that of diffusion of these substances through parchment paper into serum.

12. Absorption in the lower ileum is greater for the organic solids of serum, and less for peptone and glucose than in the upper ileum. The relative absorption of water in the upper and lower ileum is variable.

13. The relative impermeability of the lower ileum to glucose disappears with removal of the epithelium.

14. Absorption in the colon is for all constituents of serum, and for peptone and glucose far less per unit of measured surface than in the middle region of the ileum.

15. The normal relative excess of salt absorption from serum over water absorption, observed throughout the intestine, is most marked in the colon, and more marked in the lower than in the upper ileum.

16. Finally it is suggested that the cell activity which causes serum to pass over to the blood is of the same nature as that involved in the orienting action of the cells upon salts in solution.

"Studies in the Morphology of Spore-producing Members. IV. The Leptosporangiate Ferns." By F. O. Bower, Sc.D., F.R.S., Regius Professor of Botany in the University of Glasgow. Received April 6,—Read April 20, 1899.

(Abstract.)

The characters used in current classifications of Ferns need strengthening. In recent years the more detailed knowledge of the prothallus has been used for this purpose, but while not denying its value in certain specific cases, the author holds that the vegetative development of the prothallus is an uncertain guide to a general classification. On the other hand the archegonium is so uniform in its character that it gives little help, the comparison of the antheridium is, however, a useful aid, though not sufficiently varied to serve in detail[*] Accordingly the sporophyte must be the main basis Its vegetative organs have lately been largely used for systematic purposes by Christ;[†] but the same objection holds here as in Phanerogams to the use of these as characters of first rank for comparison. An attempt has therefore been made in this memoir to strengthen the characters derived from the sorus by a fresh examination of its details, and certain of its features will now be used for purposes of general comparison, which have hitherto received too little attention, they are:—

1. The relative time of appearance of sporangia of the same sorus.
2. Certain details of structure of the sporangium, including its stalk.
3. The orientation of the sporangia relatively to the whole sorus.
4. The potential productiveness of the sporangium as estimated by its spore-mother cells, and the actual spore-output

Observations of these features extending over all the more important living genera, coupled with data of habit and the characters of the Gametophyte as collateral evidence, have led the author to divide the homosporous Ferns thus:—

Simplices	Marattiaceæ	Eusporangiate
	Osmundaceæ	
	Schizæaceæ	
	Gleicheniaceæ	
	Matonineæ	
Gradatæ	Loxsomaceæ	
	Hymenophyllaceæ	Leptosporangiate
	Cyatheaceæ	
	Dicksonieæ	
	Dennstaedtiinæ	
Mixtæ ...	The bulk of the Polypodiaceæ	

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[*] Heim, 'Flora,' 1896, p 355, &c.

[†] 'Die Farrnkrauter der Erde,' Jena, 1897

These divisions are primarily based on the order of appearance of the sporangia in the sorus, the Simplices having all the sporangia of the sorus formed simultaneously, the Gradatæ having them disposed in basipetal succession, and the Mixtæ having the sporangia of different ages intermixed. But it is found that other important characters run parallel with these: thus the Simplices and Gradatæ have an oblique annulus (where definitely present), the Mixtæ (with very few exceptions) have a vertical annulus. None of the Mixtæ have been found to have a higher spore-output per sporangium than sixty-four, but this number is exceeded by some of the Gradatæ, and large numbers are the rule in the Simplices. The Simplices and Gradatæ have relatively short thick stalks, the Mixtæ usually have long and thin stalks. The orientation of the sporangia in the Simplices and Gradatæ is usually definite, in the Mixtæ it is indefinite. The receptacle is often elongated in the Gradatæ, but not in the Simplices or Mixtæ. The sum of these characters, which for the most part run parallel to one another, appears to give a substantial basis to the classification.

Evidence as to the transition from type to type has been collected. In the case of the transition from a simultaneous to a successive sorus it does not amount to a demonstration, but it is specially pointed out how slight a step it is from such a sorus as that of *Gleichenia dichotoma* to that of an *Alsophila* that given a basal indusium and marginal position, the similarity of sporangial structure and dehiscence between *Gleichenia* and *Loxsoma* is suggestive; as also the sporangial structure and high spore-output in *Hymenophyllum*. Though we may recognise these lines of similarity, they do not focus upon any one genus as the actual transitional link from the simultaneous to the basipetal. But the transition from the basipetal to the mixed sorus can be followed in detail; intermediate steps are seen in the Dennstaedtiinæ, while the fully mixed type is seen in the closely allied *Davallia*. Probably this is only one of several such lines of transition from the basipetal to the mixed type.

It is shown that a biological advantage would be gained by the suggested transitions. In the Simplices the few sporangia are large, and, arising simultaneously, make a demand all at once on the nutritive resources of the part. In the Gradatæ the smaller sporangia are produced in succession upon an elongating receptacle, and the drain on the part is spread over a longer period. But with the assumption of the mixed character the drain may be spread over an equally long time, while, as the elongated receptacle disappears, the surface from which nourishment can be derived is enlarged, and the distance through which it has to be transferred is shortened. Thus it appears biologically reasonable that the succession should be as suggested.

It is shown how the various types of dehiscence, and the action of the annulus stand in close relation to the orientation of the sporangia, and to their arrangement in the sorus. Thus the position of the annulus, which has played so important a part in classification, has been placed upon a footing of adaptation

Estimates of numerical output of spores per sporangium have been made with a view to illustrating the relation of the Eusporangiate and Leptosporangiate ferns in this respect The estimated output in the Marattiaceæ has been shown to be high,[*] that of the Polypodiaceæ is sixty-four or less. The result of numerous countings is to show that, of all Leptosporangiate ferns, *Gleichenia* approaches most nearly to the Marattiaceæ (*Gl. flabellata* may produce over 800 per sporangium); *Osmunda* may have over 500, and *Lygodium* 256 The most interesting results were derived from the Hymenophyllaceæ, in which *Hym. tunbridgense* may have over 400, while species of *Trichomanes* may produce as few as thirty-two per sporangium These results, when taken with those derived from the filmy *Todeas*, make it seem probable that the filmy habit is a condition leading to reduction of output per sporangium, and indicate that the Hymenophyllaceæ are a derivative series of reduction.

A most important commentary upon the classification proposed is derived from comparison of the antheridia, which Heim[†] found to be the most dependable part of the Gametophyte for comparative purposes He recognises two types according to their dehiscence: the one type includes, with the exception of two genera of Schizæaceæ, our Simplices and Gradatæ, while the other includes the Mixtæ. I can only regard this correspondence of parts, so aloof from one another as the antheridium and the sporangium, as establishing the relations of the Simplices and Gradatæ upon a firmer footing, the facts also give substantial support to the distinction of the Gradatæ and Mixtæ.

The effect of the observations and comparisons in this memoir is rather confirmatory of the current classifications than disturbing. The divisions suggested would supersede those of Eusporangiatæ and Leptosporangiatæ, though these terms would still be retained in a descriptive sense. If the sub-orders Osmundaceæ, Schizæaceæ, and Marattiaceæ be transferred from the end of the Synopsis Filicum to the beginning, and grouped with *Gleichenia* and *Matonia*, we have the "Simplices" before us. The Gradatæ include the Cyatheaceæ, Dicksonieæ (*Excl. Dennstaedtia*), Hymenophyllaceæ, and Loxsomaceæ, sequences probably of distinct descent, and, in my view, derivative from some prior forms such as the Simplices; and in the arrangement of Sir Wm Hooker they hold a position following on the Gleicheniaceæ. The family of Dennstaedtiinæ, founded by Prantl to include *Dennstaedtia* and *Microlepia*, also has its place here, but it leads on by intermediate steps to undoubtedly mixed forms such as *Davallia*,

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[*] 'Studies,' No. 3, p. 60.

[†] 'Flora,' 1896, p. 355, &c.

Cystopteris, *Lindsaya*, and the Pterideæ. But this sequence is already laid out in this order in the Synopsis, and it illustrates one at least of the lines along which mixed forms are believed to have been derived from the Gradatæ. No attempt has been made to follow the natural grouping of the Mixtæ into detail, or to test the arrangement of them in the Synopsis. Sufficient has, however, been said to show that the systematic divisions of the ferns now proposed fall in readily with the system of Sir William Hooker, notwithstanding that they are based upon details of which he cannot have been aware.

"Note on the Fertility of different Breeds of Sheep, with Remarks on the Prevalence of Abortion and Barrenness therein."

By WALTER HEAPE, M.A., Trinity College, Cambridge. Communicated by W. F. R. WELDON, F.R.S. Received March 9,—Read April 20, 1899.

The importance of fertility as a factor in the survival of a species is admirably demonstrated by Hafkine,[*] whilst Professor Karl Pearson[†] shows that fertility when correlated with other characteristics works a progressive change, and that not only is fertility a race characteristic, but may be a class characteristic in the human species.

Among domesticated animals, although fertility may be a racial characteristic, its importance may be much reduced from a variety of circumstances.

Among sheep there is undoubted evidence of the racial character of fertility, but the quality of the wool or the value of fat sheep of a particular breed may render that breed worth keeping in spite of a low rate of fertility as compared with other breeds. Then the rate of fertility may be artificially increased, as when certain rams of undoubted value as progenitors, but useless as breeders if left to themselves, become valuable sires by the help of the shepherd. In the same way a certain breed of sheep, kept in one district and managed in a particular manner, may be more liable to abortion, or to barrenness, or to mortality among the lambs, than the same breed in another district managed in another way, and yet the former may, on account of the supply of food which it is possible to grow there per acre, prove the most remunerative.

From these and many other similar reasons the survival of a particular breed or its retention in, or importation to, a particular district is not necessarily due to natural fitness or adaptability. At the same time a wider knowledge of the racial character of fertility among sheep might exercise considerable influence on the survival of certain breeds in the future. Just as the number of Long-horn cattle is rapidly becoming reduced in this country on account of the length of time they require to come to maturity and fatten, so the low fertility of Southdowns if it cannot be increased may lead to the retention of that breed in the hands of only a few special breeders.

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[*] "Recherches sur l'adaptation au milieu chez les Infusoires et les Bactéries; contribution à l'étude de l'immunité," 'Annales de l'Institut Pasteur,' vol 4, 1890.

[†] "Contributions to the Mathematical Theory of Evolution. Note on Reproductive Selection," 'Roy. Soc. Proc.,' vol. 59, 1896.

The following account is a brief abstract of information obtained from 397 sheep breeders who have supplied me with records of flocks containing 122,673 ewes for the breeding season of 1896-97. Table IV records certain particulars of eight pure breeds separately, and of ten pure breeds jointly ("Various Pure Breeds"; details of only a small number of flocks were supplied for each of these ten breeds). These are totalled, particulars of fifty-nine flocks of various cross-bred sheep subjoined, and the totals for all breeds finally arrived at.

Besides the figures contained in Table IV, other statistics were supplied regarding the age of rams and ewes, the size of breeding flocks, the proportions of rams and ewes therein, the usual and the highest percentages of barrenness experienced, &c To this were added remarks on the food given to rams and ewes at specified times, on their condition during the breeding season, on various methods of management of breeding flocks, and on numerous views regarding matters which are supposed to influence fertility, abortion, and barrenness amongst ewes; finally particulars of districts, subsoils, and weather were noted, and the subject considered statistically from all these various points of view for each breed.

The variation in the numbers of flocks and of ewes concerned in the various calculations for each breed (see Table IV) is due to the inability of all flock-masters to supply the whole of the information asked for in the schedule, which, by the kindness of the Royal Agricultural Society, was distributed to them, and is not due to my selection of flocks

Fertility

It is to be noted the number of lambs returned does not always represent the number born; that information, although asked for, was not always forthcoming and sometimes the number of lambs alive when the schedule was filled up was given instead. The percentage of twins given is, however, a check on this element of error, and in the following account the columns under "lambs" and "twins," Table IV, are considered together. The error is most apparent in the Hampshire Down, Oxford Down, Dorset Horn and Lincoln breeds; in the other four breeds, in which the records have been most carefully kept, the Suffolk, Shropshire, Kent and Southdown breeds, their fertility is demonstrated to be a racial character.

The Suffolk breed is by far the most fertile, while the Southdowns are at the bottom of the list: the value of the former as a prolific breed is incontrovertible, while the record of the latter, as shown both by the percentage of twins and lambs, is so low as to suggest cause for some anxiety and to show urgent need for close attention on the part of breeders.

The Hampshire breed also stands low both with regard to twins and lambs, especially when its low percentage of loss from abortion and barrenness is considered, whereas the Lincoln breed, although recording only 111 1 per cent of lambs, shows 29 09 per cent. of ewes bearing twins, and the fault, with this breed, is obviously not so much low fertility as heavy mortality among lambs and a high percentage of loss from abortion and barrenness

The most fertile of all pure breeds of which I have records, is the Wensleydale breed, in which six flocks with a total of 319 ewes produce 177·43 per cent of lambs (included in "Various Pure Breeds"). It is to be noted that both Wensleydale and Suffolk ewes when covered by rams of other breeds, do not appear to produce a larger percentage of lambs than they produce when covered by rams of their own breed; if anything, they seem to produce a somewhat smaller percentage of lambs. On the other hand, Dorset Horn ewes are more usually fertile and are more prolific with Down rams than with Dorset Horn rams.

It is a very usual practice with Dorset Horn flock-masters to keep a Down ram to cover those ewes which fail to become pregnant to a Dorset Horn ram, and, when pure-bred ewes are discarded from their breeding flocks, they generally sell them "*in young*" to Down rams, when they frequently produce as many as 170 per cent. of lambs.

It appears, therefore, that whereas the fertility of Suffolk and Wensleydale ewes is at its maximum when they breed to rams of their own kind, Dorset Horn ewes require to be covered by rams of another breed, in order that they may be stimulated to the greatest generative activity.

Apart from the percentage of ewes which abort or are barren from accident or constitutional defects, the fertility of ewes is chiefly affected by their condition and by the condition of the ram at tupping time. I have overwelming evidence that flocks in strong condition at tupping time produce a higher percentage of lambs than flocks in poor condition at that period (Suffolk, Kent, Hampshire, Dorset Horn, and Lincoln breeds); on the other hand, fat rams and ewes are associated with a high percentage of barrenness, and not with a high percentage of lambs.

In close connection with this subject is the production of twins. Fifty-five per cent. of the flock-masters who send information on this head, report that twins are usually born early in the lambing season, and many of them add that otherwise the crop of lambs is small. That is to say, that most twins are produced by the ewes which first come in season, and I interpret that to mean that it is the ewes with the most active and most vigorous generative system, those which are in strong breeding condition at that time, which bear the most twins. It is of interest to note that among Southdown and Hampshire flocks, a comparatively small proportion of twins are born early in the lambing season, and these breeds produce the smallest percentage of twins.

The effect of locality upon the fertility of a breed is worthy of consideration, and statistics are given in Table V to illustrate this point.* In considering these figures, numerous other important influences must be borne in mind, which the information at my disposal makes it impossible to dissociate from locality, and in all cases, except those of the Shropshire and Lincoln breeds, where a wide difference in fertility is shown in different districts, it is possible that the variation in fertility may be due to one or other or all of these various influences, apart altogether from locality.

The fertility of a flock is greatly influenced by its management and by the conditions under which it lives, the condition, kind, and amount of food available before tupping time will affect the condition of the ewes and the percentage of twins subsequently born, the season may be more favourable for tupping or for lambing in one district than in another, cold, rain, or want of rain, will affect the feed, the ground, and the ewes themselves, while, owing to mortality among ewes during lambing or at other times in the preceding year, the flocks in one district may consist of a larger proportion of shearling ewes than the flocks in another district, and this may affect the birth rate of a flock.

These and numerous other such influences, combined with the undue proportionate value of excessive loss or fertility in one flock, where only a few are kept in a district, is sufficient to account for a much greater variation than is shown for most breeds in Table V. At the same time the difference in fertility of Shropshire flocks kept in Staffordshire, as compared with other flocks of the same breed kept elsewhere, and of Lincoln flocks kept in Yorkshire, as compared with those kept in the home county, is very remarkable. They certainly suggest that Yorkshire is a more satisfactory habitat for the Lincoln breed than is the home county, and although it is quite possible the method of farming and other influences, in Lincolnshire, are responsible for some of the difference, they can hardly be responsible for all of it. I do not believe that the high percentage of loss in the Lincolnshire wold flocks is due altogether to mismanagement, but if 50 per cent. of the difference between the losses of the wold flocks and the Yorkshire flocks was added to the percentage of twins of the former, there would still be a difference of 17 per cent. of twins in favour of the Yorkshire flocks. So also with the Shropshire breed in Staffordshire and the home county, if 50 per cent of the difference between the losses was added to the percentage of twins in the home county, they would still have 12 per cent. less than the Staffordshire flocks, and it is not probable that a different method of farming in these two neighbouring districts can account for such difference in fertility.

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* See "Percentage of Twins."

The returns of the Suffolk flocks in Essex are misleading, inasmuch as some of the flocks with the highest percentage of lambs do not show the percentage of twins, at the same time I am disposed to think that Essex is not so favourable a county as Suffolk for this breed of sheep

The difference in the return of lambs for Southdowns in East Anglia and in the South, is probably due to the more careful records which the smaller size of the flocks and the method of farming in East Anglia allows, and I suspect the difference in the percentage of twins may be similarly accounted for

In neither of the cases where different breeds are represented in the same locality, does the percentage of twins of the foreign flocks approach that of the home breed Southdowns do not approach the fertility of Suffolks in East Anglia, nor do Hampshires become as fertile as Dorset Horns in the West country.

The Suffolks, Hampshires, and Dorset Horns are most fertile in their home districts; in the case of the Shropshire breed, Staffordshire may be considered a part of the home district, and only in the case of the Lincolns is it demonstrated that any breed thrives better in a foreign district than at home. This was to be expected, for it can hardly be doubted that natural selection, as well as artificial selection, has been at work on the different breeds of sheep in this country.

In the case of the Lincoln breed it is of interest to note, in this connection, that the method of farming in some parts of Lincolnshire has greatly altered in modern times; for instance, the facility which the soil on the wolds affords to grow especially fine crops of roots, enables the flock-masters in that district to keep more sheep per acre than is possible on the low-lying farms; this leads to crowding and to other even more artificial conditions which, as the statistics indicate, are not favourable to the fertility of the breed, on the other hand several of the more successful flocks in Yorkshire are run on grass chiefly and under more natural conditions for sheep.

The variation in fertility in different districts does not, however, affect the racial character of the fertility of the different breeds (compare Tables IV and V—"Per cent. of Twins"), except in the case of the Lincolns, where the returns of the Yorkshire flocks suggest that this breed should be placed in the first rank with the Suffolks and Shropshires, instead of in the second rank with Dorset Horns, Oxford Downs, and Kents. The position of the Southdowns at the bottom of the list remains unaltered.

While the percentage of lambs and of twins in cross-bred flocks is greater than the same percentage for the total pure-bred flocks, the ewes of certain pure breeds are undoubtedly more fertile than the average cross-bred ewe

The flock percentage of lambs in 306 pure-bred flocks, ranges from 203·8 to 59 09 per cent, the percentage for 89,370 ewes being 120·4 per cent. The most frequent percentage in these 306 flocks is between 110 and 120 per cent., the following Table (I) shows this, and as the column for "under 110 per cent." includes all failures, the excess in the 110 per cent column is all the more marked.

There are more flocks of between 100 and 200 ewes than of any other number, and it is in these flocks the highest percentage of lambs occur, broadly speaking, the frequency of a high percentage of lambs, and the height of that percentage, vary in proportion to the number of flocks and to their size.

Abortion and Barrenness.

There is an element of error in these statistics, due to the fact that some ewes abort at an early stage of gestation, when the fœtus is small and the circumstance liable to be overlooked by the shepherd; some of these ewes come "in use" again and are again served by the ram, but some do not again come "in use," or, owing to the fact that they have already been drafted into a flock without a ram, they are not again covered, in either of these cases they are put down as barren. Thus, although I do not believe the error is a great one, the percentage of abortion in ewes may be higher and the percentage of barrenness lower than is represented in Table IV.

The total loss from both these causes amounts to 7 1 per cent. for all pure-bred ewes, of these the Lincoln sheep suffer the most (12 per cent.) and the Hampshires the least (4·01 per cent), a very startling difference and of specially grave significance to Lincolnshire flock-masters on the wolds (see Table V).

Abortion.

In 300 pure-bred flocks the percentage of abortion varies from 23 75 per cent to 0, while the percentage for 85,878 ewes is 2·39. The Dorset Horn (4·11 per cent.) and the Lincoln (4 per cent.) breeds suffer most, while all other breeds except Southdowns have less than 2 per cent. The causes which induce a high percentage of abortion are little understood, and severe losses are from time to time experienced from no known cause (Lincolns 20 to 30 per cent. and Dorset Horns).

Statistics supplied indicate, that an undue proportion of shearling

Table I.—Total Pure-bred Flocks. Lambs—per 100 ewes.

Flocks of	Under 110 p c	110 p c	120 p c.	130 p c.	140 p c	150 p c	160 p c.	170 p c.	180 p c	190 p c	200 p c and over	Totals
Under 100	1	7	10	6	8	6	7	4	1	1	..	51
100—199.............	7	17	11	4	10	12	8	2	2	1	1	85
200—299...........	9	16	12	6	10	8	..	1	..	..	..	62
300—399.............	13	12	7	3	4	1	..	..	..	..	..	40
400—499	9	9	3	4	2	..	..	..	.	..	..	27
500—599...........	1	3	5	1	..	..	.	..	..	..	.	10
600—699.............	3	1	2	2	..	..	..	..	..	..	..	8
700—799.............	3	1	..	2	..	..	..	..	..	..	..	6
800—899.............	3	..	1	1	..	..	..	..	..	..	.	5
900—999......... ...	..	..	1	..	..	..	..	..	..	..	.	1
1000 and over	4	3	3	1	..	..	..	.	..	..	.	11
Total	53	69	55	40	34	27	15	7	3	2	1	306

ewes in a flock is associated with a high percentage of abortion (Lincolns), and that ewes of a particular breed run on certain subsoils or in certain districts are more liable to abortion than the average for that breed, as, for instance, Lincolns run on the wolds and Hampshires on oolite formation.

In some parts of Lincolnshire abortion sometimes approaches, if it does not actually assume, an epidemic form; at such times several neighbouring flocks may experience between 30 and 40 per cent of aborted ewes, I am unaware of a similar form of abortion in any other district.

Unsuitable food, causing indigestion and intestinal irritation, and poor food, resulting in poor nutrition, are probably responsible for the greatest proportion of abortion in ewes It is not the kind of food, as is frequently supposed, but the condition of that food which is at fault, and, as a result, my schedules show that poor condition of ewes during gestation is undoubtedly associated with a relatively high percentage of abortion.

The highly artificial conditions under which sheep are kept in many districts in this country, renders the question of the most suitable food for breeding ewes a very important question, and it is one regarding which but little attention has been paid.

The most frequent percentage of abortion experienced in 300 pure-bred flocks is shown in Table II to be under 1 per cent. The highest percentage is relatively more frequent in large than in small flocks, and this table shows that much more irregularity is experienced in abortion than Table I shows is the case for fertility.

Barrenness.

In 327 flocks of pure-bred ewes the percentage of barrenness varies from 51·42 per cent. to 0, while the percentage for 96,520 ewes is 4·71. The Lincoln (8 per cent.) and the Shropshire (6 06 per cent.) breeds suffer the most, while, with the exception of Hampshire Downs, Dorset Horns, and Suffolks, no pure breeds record less than 5 per cent. loss from this cause

The district or subsoil on which ewes are run is associated, in certain breeds, with the proportion of barrenness experienced. thus, Lincoln sheep run on the wolds, Shropshire sheep on new red sandstone subsoil, and Hampshire sheep elsewhere than on chalk downs, are associated statistically with a relatively high percentage of barrenness; an excessive proportion of shearling ewes in a flock is also frequently found associated with a high percentage of barrenness; but the quality of the food given and the condition of the rams and ewes at tupping time is no doubt the chief factor which influences the barrenness percentage.

Fat is well described as an enemy to fruitfulness, while the want of

Table Total Pure-bred Flocks.—Abortion.

Flocks of	Under 1 p c.	1 p. c.	2 p c.	3 p c	4 p c	5 p c	6 p c.	7 p c	8 p c	9 p c	10 p. c and over	Total
Under 100	14	14	6	3	3	2	2	1	..		.	45
100—199	45	20	12	5	1	.	2	1	..	..	2	88
200—299	31	17	8	1	4	1	..	..	..	1	1	64
300—399	20	11	4	..	1	1	1	1	..	1		40
400—499	9	7	5	2	1	.	..	..	..	..	2	26
500—599	5	..	1	.	..	..	..	..	..	..	..	6
600—699	4	4	..	1	..	..	..	..	..	..	2	11
700—799	2	1	..	1	..	..	..	..	..	..	1	5
800—899	2	..	..	..	..	..	..	..	..	..	2	4
900—999	.	..	..	..	..	..	..	..	..	..	..	..
1000 and over	2	4	3	1	.	.		..	..	..	1	11
Total	134	78	39	14	10	4	5	3	.	2	11	300

Table III.—Total Pure-bred Flocks.—Barrenness.

Ewes in flocks of	Under 1 p c	1 p. c.	2 p c.	3 p c	4 p c	5 p. c.	6 p c	7 p c	8 p c	9 p c	10 p c and over	Totals
Under 100	16	11	3	5	3	5	3	..	1	1	3	51
100—199	16	21	12	8	3	9	5	4	5	1	11	95
200—299	6	11	13	8	5	7	4	3	2	1	6	66
300—399	9	6	7	2	5	1	..	2	2	..	7	41
400—499	6	7	..	3	2	3	2	.	1	1	3	28
500—599	1	2	1	1	..	3	..	1	..	1	..	10
600—699	1	1	1	4	2	2	..	..	.	..	..	11
700—799	1	..	1	..	.	2	1	1	.	..	..	6
800—899	..	..	..	2	..	1	1	..	..	1	1	6
900—999	..	..	..	..	..	1	..	..	..	..	..	1
1000 and over	3	2	1	..	..	3	..	2	1	..	..	12
Total.........	59	61	39	33	20	37	16	13	12	6	31	327

Table IV.

Breed.	No of flocks	No of rams	No of ewes	Lambs				Twins				Abortion				Barrenness				Total loss aborted and barren
				No of flocks	No of ewes.	No. of lambs.	Lambs per 100 ewes	No of flocks	No of ewes	No. of ewes bearing twins	Per cent twins	No of flocks	No of ewes	No of aborted ewes	Per cent aborted	No of flocks.	No of ewes	No of barren ewes	Per cent barren	
1. **Suffolk** . ..	38	161	7,506	38	7,170	10,165	141·77	16	2,553	1,440	56·40	36	6,861	92	1·34	36	7,140	234	3·28	4·62
2. **Kent**	15	254	9,931	13	8,481	10,521	124·05	11	6,703	2,104	31·38	9	3,901	54	1·38	15	9,931	549	5·52	6·90
3. **Southdown**	23	186	9,134	22	7,834	8,609	109·89	18	6,583	1,229	18·67	21	8,894	255	2·86	23	9,134	464	5·08	7·94
4. **Hampshire**	53	473	26,400	50	24,860	28,512	114·69	41	21,141	5,093	24·09	48	23,755	371	1·56	51	25,100	615	2·45	4·01
5. **Oxford Down**	20	83	3,575	18	3,189	3,800	119·16	14	2,601	911	35·02	16	2,685	36	1·34	20	3,555	180	5·06	6·40
6. **Dorset Horn**	31	170	10,265	25	8,163	10,092	123·63	27	8,588	3,225	37·55	28	9,020	371	4·11	29	9,408	273	2·9	7·01
7. **Shropshire** .	60	196	8,492	56	8,014	11,001	137·27	46	4,131	1,932	46·77	52	7,426	112	1·50	58	7,882	478	6·06	7·56
8. **Lincoln**	62	367	17,480	51	15,789	17,542	111·10	46	11,430	3,326	29·09	59	16,697	665	4	60	16,570	1,325	8	12
9 to 18. **Various pure breeds**	36	195	10,010	34	5,840	7,358	126	25	4,513	1,268	28·09	31	6,636	93	1·4	35	7,810	436	5·58	6·98
Total pure breeds	333	2,085	103,193	308	89,370	107,603	120·40	237	68,535	20,578	30·02	300	85,678	2,052	2·39	327	96,720	4,554	4·71	7·10
19. **Cross-breds** .	59	415	19,480	52	12,165	15,751	129·47	33	9,314	2,891	31·04	50	11,361	173	1·52	57	18,090	588	3·25	4·77
Total all breeds	397	2,500	122,673	358	101,535	123,354	121·48	275	77,850	23,469	30·14	350	97,239	2,225	2·28	384	114,810	5,142	4·48	6·76

Table V

Breed.	Locality	Number of flocks	Number of ewes	Per cent of lambs	Per cent of twins	Per cent of loss, abortion, and barrenness	Remarks
Suffolks	Suffolk	28	5,493	141·16	60·46	4·5	Some of flocks with highest per cent of lambs did not return per cent of twins
	Essex	8	1,458	146·16	42·87	5·21	
Southdowns	South (Hants, Surrey Sussex)	13	6,278	109·51	19·71	7·42	Small flocks
	East Anglia (Norfolk, Suffolk, Essex, Cambridgeshire)	6	1,745	119·02	22·62	7·17	
Hampshires	South (Hants)	15	5,465	108·73	27·85	4·2	
	West (Dorset Wilts, Somerset)	27	18,621	116·3	22·4	3·7	
Dorset Horns	West (Dorset)	19	6,580	122·46	38·49	6·88	
	Isle of Wight	4	1,497	130·67	36·07	12·7	One flock had excessive barrenness.
Shropshires	Shropshire	22	2,908	134·2	40·8	10·43	Exceptionally heavy losses from barrenness
	Staffordshire	9	1,265	154·15	54·97	6·08	
	Herefordshire	9	1,039	133·51	33·09	5·29	
Lincolns	Lincoln (Wolds)	16	6,813	101·33	23·75	17·78	Heavy losses from both abortion and barrenness
	,, (elsewhere)	29	8,091	115·58	24·13	7·79	
	Yorkshire	9	1,629	127·26	47·57	4·78	

***Note.*—The Kent flocks are all run in Kent. The Oxford Down flocks, in my records, are so scattered, that a sufficient number of flocks for the purpose of comparison is not to be found in any two districts.**

sufficiently nutritious food also results in poor returns of lambs. Among Suffolk and Shropshire ewes, which are highly fed as a rule, a high percentage of barrenness occurs in cases where they are excessively highly fed; on the other hand, among Dorset Horn, Lincoln, and Kent ewes, which are certainly not too highly fed as a rule at tupping time, the highest percentage of barrenness occurs among the poorest kept flocks.

The most frequent percentage of barrenness experienced in 327 flocks is 1 to 2 per cent., but, as Table III shows, the returns are much more irregular than was the case for abortion, and there is a much larger proportion of flocks in the "10 per cent. and over" column. The slightly excessive proportion of flocks in the 5 per cent. column suggests generalised results rather than accurate returns, but the number of flock-masters who are responsible for this is obviously very small.

In conclusion.—

1. Whereas the total loss from abortion and barrenness is fairly constant for most pure breeds of sheep, the Suffolks and Hampshires are markedly free from, and the Lincolns markedly liable to, heavy loss from these causes.
2. Although the loss from the above causes does exert an influence on the returns of fertility of the various breeds, it does not account for the wide variation which exists in this respect.
3. The ewes of certain pure breeds are conspicuously more fertile than the average cross-bred ewe; and
4. The fertility of certain pure breeds is sufficiently marked to constitute a racial characteristic.

"Some further Remarks on Red-water or Texas Fever." By ALEXANDER EDINGTON, M.B., F.R.S.E., Director of the Bacteriological Institute, Cape Colony. Communicated by Dr. D. GILL, C.B., F.R.S.

Received March 13,—Read April 20, 1899.

Since my communication* to the Royal Society of London, by Professor Thomas R. Frazer, I have been able to obtain valuable addi-

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* The conclusions arrived at in that communication (received June 6, 1898) were as follows:—

1. The blood of animals, themselves healthy, from a red-water area is dangerous if inoculated into an animal which suffers coincidentally from another disease.
2. That the blood of animals suffering from mild or modified red-water may be safely used to inoculate a healthy animal *subcutaneously*, but is dangerous when injected into a vein.
3. That the subcutaneous inoculation of mild or modified red-water blood conveys a mild form of the disease, and since the blood of such an animal is viru-

tional evidence as to the communicability of the disease by the use of blood derived from animals which have been either recovered from the sickness for a very considerable time or which have been inoculated many months previously to the date on which their blood has been used.

On the 8th December, 1898, I withdrew some blood from animal No 18, which has been continuously under observation since it was inoculated on the 22nd December, 1897 After defibrinating the blood, 20 c c was used to inoculate a young ox (No 54) by intravenous injection On the following day a sharp rise of temperature occurred, which reached to 106 6 F On the following morning it was observed to have fallen to 99 8 F. Three days later the temperature was again over 104° F, but fell previous to the next morning From this time onward an erratic course of temperature was observed, and on the twenty-fifth day, subsequent to inoculation, it was seen to be ill, refused food, but had no definite symptoms of "red-water" Three days later it died. The blood on examination was seen to contain the spherical forms of the parasite.

On post-mortem examination, the bladder and urine were quite normal The liver was not enlarged, but was somewhat discoloured in patches, and the biliary ducts were distended with bile The bile was much altered, being stringy and of a greenish-yellow colour. The spleen was normal in size and consistence The kidneys were enlarged and the pelves were filled up by a yellowish gelatinous exudation. The cortex was somewhat congested, but there was no evidence of any true inflammatory change The general muscles were pale in colour, and there was slight evidence of jaundice. This experiment serves to show that an animal which has been inoculated with infected blood, while it may not develop much illness as a result of it, is really infected and, moreover, its blood, if drawn as late as a year subsequently, is yet so infective that an intravenous injection of it, into susceptible

lent when injected into a vein in another animal, it is safely to be inferred that the animal suffering from the mild form becomes more or less immunised or "salted"

On these grounds I would suggest a method of protective inoculation against red-water in the following manner Having procured a healthy animal from a red-water area, or one which is known to have been "salted," inoculate it by injecting 5 c c of red-water blood into the jugular vein and 5 c c. subcutaneously. In cases where the operator is unable to attempt the vein inoculation I would recommend the subcutaneous inoculation of 5 c c in four different sites

Allow at least twenty-eight days to elapse, and if any degree of illness is recognised, the blood of this animal may be used, after being defibrinated, to inoculate healthy cattle. For such inoculation only 5 c c should be injected into small animals and not more than 10 c c. into larger

Seeing, however, that the presence of other maladies renders such a proceeding unsafe, I would recommend that it should only be practised during the autumn or winter, when the veld diseases are, as a rule, in abeyance, and in no case when any epidemic disease is in the near neighbourhood.

animals, will certainly infect, and may even kill, although after a somewhat extended period of time.

Very important corroboration of this is furnished by the experience of inoculation for red-water, which has lately been adopted in the Cape Colony. Four animals which were immune to red-water (three by reason of having had the disease and recovered, and one by being born and reared on permanently infected veld) were sent from Fort Beaufort to Queenstown to be used by the veterinary surgeon there for inoculation purposes. The animals to be used for inoculation had been "fortified," *i.e.*, re-inoculated with virulent blood, seven weeks previously.

Twenty animals were inoculated with defibrinated blood from one animal, the doses used being 10 to 20 c c, according to age All had a febrile reaction and some slight symptoms of the sickness, but easily recovered. From one of the other of the four animals blood was taken and used to inoculate seven head, giving doses of 10 to 15 c.c. These also all had a reaction, but made good recovery.

On November 1st the four animals were re-inoculated with virulent red-water blood, and in each case 5 c c. was injected intravenously and 10 c.c subcutaneously. Twenty-nine days later hey were bled. With this blood two lots of cattle were inoculated.

One lot consisted of 107 animals which had not ever been exposed to red-water infection. The doses used were increased beyond those which I had recommended, namely, 10 to 25 c c. were used, according to age. Of these animals no less than seventeen died of characteristic red-water. The remainder made a good recovery.

The second lot consisted of fifty-three head of cattle, all of which, with one exception (an imported animal) had been born and reared on red-water veld The imported animal was the only one which showed any signs of reaction, but it made a good recovery.

This experience has sufficed to show that it is not always safe to exceed the doses which I have recommended, unless the animals which have been used for withdrawing blood have been untouched for at least a considerable number of months.

I have been able, with the co-operation of several farmers, to carry out experiments by which inoculated cattle have been fully exposed to infection at later dates. In May, 1898, I inoculated ten head of old cattle with blood from an animal which had been inoculated, six months previously, with virulent blood. These cattle were immediately removed from the Institute, and later sent to an infected area in company with ten head of young animals which were uninoculated, but, as is commonly known in this colony, are not so liable to death from this disease as are older animals. Of the young stock all have been infected by exposure in the veld, and three have died. Of the older, more susceptible, animals not one has shown the slightest signs of illness, and the cows have given birth to healthy calves.

Mr. J. H. Webber had twenty-eight head of Fish River cattle inoculated on the 7th November, 1898, and subsequently had them removed to his farm, which is well known to be one of the worst infested areas in the eastern province. Previous experience has shown that if clean cattle are placed there they become very quickly affected with the disease On the 5th December one died from gall-sickness, but, with this exception, all have done very well, and are at this date in perfect health.

This method of inoculation has proved so satisfactory to the farmers themselves that it is being very generally adopted, and the farmers have petitioned the Government to arrange for an inoculating station being placed at Graham's Town, so that clean cattle coming from clean Karroo areas for transmission to the coast may be inoculated previous to entering the infested belt.

April 27, 1899.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them

The following Papers were read:—

I. "Data for the Problem of Evolution in Man. I. A First Study of the Variability and Correlation of the Hand." By Miss M. A. WHITELEY, B Sc., and KARL PEARSON, F R S

II. "On the Luminosity of the Rare Earths when heated *in Vacuo* by means of Cathode Rays." By A. A. CAMPBELL SWINTON. Communicated by LORD KELVIN, F.R.S.

III. "On a Quartz-thread Gravity Balance." By RICHARD THRELFALL and J A POLLOCK. Communicated by Professor J. J. THOMSON, F R.S.

IV. "On the Electrical Conductivity of Flames containing Salt Vapours." By HAROLD A. WILSON, B.Sc. Communicated by Professor J. J. THOMSON, F.R.S.

V. "On a Self-recovering Coherer and the Study of the Cohering Action of different Metals." By Professor JAGADIS CHUNDER BOSE, M.A., D.Sc. Communicated by LORD RAYLEIGH, F.R.S.

VI. "On the Presence of Oxygen in the Atmospheres of certain Fixed Stars." By DAVID GILL, C.B., F.R.S.

"On the Luminosity of the Rare Earths when heated *in Vacuo* by means of Cathode Rays." By A. A. CAMPBELL SWINTON. Communicated by LORD KELVIN, F.R.S. Received March 20, —Read April 27, 1899.

For incandescent gas mantles, it is found that certain definite mixtures of the rare earths are necessary, in order to obtain the maximum luminosity. For instance, in the ordinary Bunsen gas flame, a mantle consisting of pure thorium oxide, or of pure cerium oxide, will only give about one-eleventh of the light that is given by a mantle composed of 99 per cent. of thorium oxide, and 1 per cent. of cerium oxide, which is the mixture at present used by the Welsbach Company.

In order to explain this remarkable fact, several different and somewhat contradictory theories have been propounded, one of which implies catalytic or other chemical action between the oxides and the constituents of the Bunsen flame.

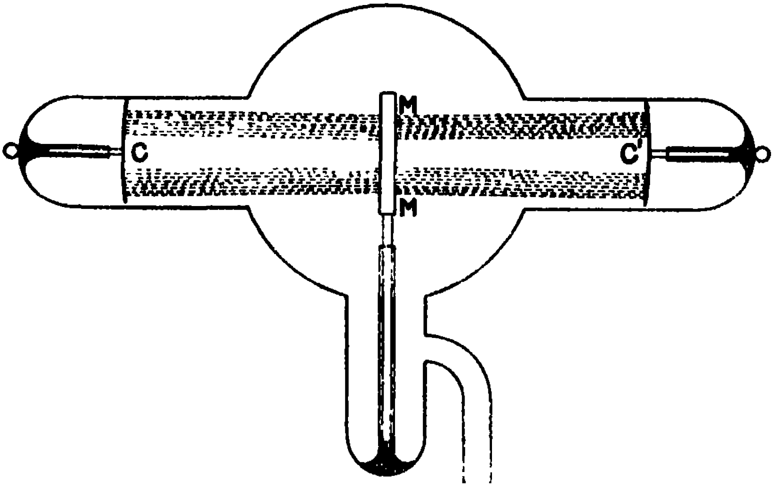
In order to investigate this question, it is obviously important to note the behaviour of the rare earths at high temperatures without contact with any flame, and endeavours have already been made to effect this by heating the oxides in specially constructed furnaces. Under these conditions, only very minute differences could be detected in the amount of light given by different oxides and mixtures, but it appears doubtful whether the very high temperature of the Bunsen flame was really attained.

It has occurred to the writer, that very high incandescence could be produced by enclosing mantles in a vacuum tube, and subjecting them to bombardment by means of cathode rays, when the mantles would not be in contact with anything except the cathode rays themselves, and the comparatively small amount of residual gas that remains in the tube at the requisite high degree of exhaustion.

Since the date of Sir William Crookes's early researches, it has been known that a very high temperature could be produced in a body placed at the focus of the convergent rays from a concave cathode. In this manner Crookes melted platinum and glass, and brought carbon wool to bright incandescence. The writer has made many experiments on this subject, using instead of the interrupted continuous currents employed by previous investigators, alternating electric currents, which appear to have many advantages for this purpose. At the Royal Institution, in February, 1898, the writer showed that very brilliant incandescence could be obtained for a short time in a small block of lime, placed in a suitably exhausted tube midway between two concave electrodes, connected to an alternating electric supply at about 6000 volts pressure, and in June, 1898, at the Royal

Society, he exhibited in action a similar arrangement, but with the block of lime replaced by a flat plate of thorium oxide. In this case the concave electrodes were of such a curvature and were placed so far apart, that the two sides of the thorium plate in each case intersected the diverging cone of cathode rays. Under these conditions nearly a square inch of the thoria surface on each side of the plate became highly incandescent, and a very powerful light was obtained for some minutes at a time, but only at a critical and highly unstable degree of vacuum.

The writer has now applied this method to the investigation of the comparative luminosity of different mixtures of the rare earths



One form of the tube employed was constructed as shown in the above figure, where C, C′ are two spherically concave discs of aluminium 1 125 inches diameter, and 6 inches radius of curvature. These electrodes are placed about 7 inches apart, and were connected to the secondary terminals of a 10-inch Ruhmkorff coil, the primary of which was supplied through a variable resistance, with alternating electric current at 100 volts pressure from the main. The tube was connected through a drying tube, containing phosphorus pentoxide, to a pair of Toepler pumps, and also to a McLeod gauge.

The mantle M M to be experimented upon, was mounted on a platinum wire frame and placed between the two electrodes, so that as the electric current alternated, and each electrode became in turn cathode, the mantle was subjected on alternate sides to cathode ray bombardment. The curvature of the electrodes was such as to give almost parallel beams of cathode rays, so that a considerable ring shaped, and slightly hollow, area on each side of the mantle was subjected to the rays, and could be brought to high incandescence.

A preliminary experiment was made with a mantle of asbestos, powdered over in patches with pure thorium oxide. With this it was found that at a suitable degree of exhaustion, the patches of thoria became brilliantly incandescent, with an intensity of cathode rays that made the asbestos barely red hot.

Experiments were next made with mantles consisting entirely of thoria and ceria, both separately, and mixed in different proportions. These mantles were prepared in a similar manner to the Welsbach incandescent gas mantles, by saturating a carefully purified cotton fabric with ammonium nitrate of thorium and cerium, and then burning out the cotton Very thick and closely woven cotton lamp wick, freed from foreign matter by treatment with caustic soda, hydrochloric acid, and ammonia, was employed in place of the thin fabric usually used, so that the resulting mantle after burning out the cotton, was very close in texture, and fully 0·2 inch thick. This was found necessary, as otherwise some of the cathode rays passed through the mantle and melted the opposite aluminium electrode

In order to obtain accurate comparisons between pure oxides and different mixtures, the mantles were made in patchwork, each complete mantle being made up of two or four sections, separately impregnated with different solutions, and then sewn together with impregnated cotton before being burnt

The mantles were so mounted in the vacuum tube that the cathode rays impinged equally upon the portions that consisted of different mixtures, so that an equal amount of energy was imparted to each sample.

With a compound mantle prepared in this way, composed one-half of pure thorium oxide, and the other half of a mixture of 99 per cent. thorium oxide plus 1 per cent. of cerium oxide, it was found after exhaustion that on starting the cathode discharge the thoria plus ceria heated up to incandescence more rapidly, and, on stopping the discharge, cooled more rapidly than the pure thoria. Further, when at full incandescence and observed through a dark glass, the thoria plus ceria was slightly more luminous than the pure thoria, though the difference was very small, probably not more than 5 per cent. Owing to the difficulty of maintaining a constant vacuum, accurate photometrical measurements were not possible, but the amount of light under favourable conditions was roughly estimated at, at least, 150-candle power per square inch of incandescent surface, this being obtained with an expenditure of electrical energy in the secondary circuit at about 8,000 volts pressure of approximately 1 watt per candle. The amount of exhaustion suited to give the best results varied with the dimensions of the tube and the conditions mentioned below, but was approximately about 0·00005 atmosphere, the maximum luminosity being obtained when the dark spaces of the two cathodes just crossed at the centre of the bulb. Owing to the large amount of gas occluded by the mantle, a proper degree of permanent exhaustion was very difficult to arrive at, and required continuous pumping for many hours with the cathode rays turned on at intervals. Even then the conditions of maximum luminosity were exceedingly unstable, owing to the further liberation of occluded gas on the one hand, and on the other to the rapid increase in the degree of exhaustion owing to absorption of the residual gas by the electrodes That such absorption probably took place in the aluminium electrodes, and not in the mantle, was demonstrated by other experiments with a tube in which there was no mantle, but only two electrodes of aluminium wire.

After the cathode rays had been allowed to bombard the mantle for a short time, the latter was found to have become discoloured where bombarded. That portion which was composed of pure thoria became dark blue, while the thoria plus ceria became brown. This effect, which appears to be analogous to those observed by Goldstein with lithium chloride and sodium chloride,* seems to be due to a partial reduction of the oxides by the cathode rays. The admission of a very minute quantity of air to the tube while the cathode rays are acting on the mantle, and the latter is in parts incandescent, causes the discoloration to disappear instantaneously on the incandescent, but not upon the cool portions, probably by re-oxidation of the partially reduced oxides, while the discoloration also slowly vanishes in a day or two with the mantle cold if air at ordinary atmospheric pressure is admitted to the tube. By continuing to bombard the mantle with cathode rays, and alternately allowing the vacuum to increase and letting in small quantities of air, the discoloration can be made to appear and to disappear over and over again as often as desired. At the moment of admitting the air, the amount of light was found momentarily to increase, this being probably due to the increased temperature due to the re-oxidation of the partially reduced oxides.

After repeating this process of letting in small quantities of air, and allowing them to be absorbed, several times, it was found that the degree of exhaustion which gave the maximum incandescence had altered from 0·000047 to 0 000112 atmosphere, as measured by the McLeod gauge. Similar effects were obtained with a tube containing no mantle, but only aluminium wire electrodes, the inference being that some change takes place in the residual gas which renders it less conducting.

At a higher degree of exhaustion than that which produced incandescence of the mantle, the pure thoria was found to fluoresce blue,

* 'Wied. Ann.,' 1895, No. 54, p. 371.

and the thoria plus ceria with a yellowish light. The fluorescence in each case was much less bright when the oxides were white than when they had become discoloured by previous bombardment With very high exhaustions the thoria plus ceria fluoresced the more brightly, at lower exhaustions the pure thoria gave the brighter fluorescence.

On the suggestion of Mr. W Mackean, the tube was pumped up to a very high vacuum and oxygen admitted. A similar experiment was made with hydrogen, the tube being completely filled with the gas, and then pumped to the proper degree of exhaustion Though at low exhaustions these gases gave distinctive appearances to the discharge in the tube, no difference in the behaviour of the mantles with them and with air could be detected when once the vacuum reached the degree required for producing incandescence of the mantle.

Further experiments were made with a similar tube containing a compound mantle made up of four sections, composed as follows —(1) pure ceria, (2) pure thoria, (3) 50 per cent. thoria 50 per cent. ceria, (4) 99 per cent. thoria 1 per cent. ceria.

With an intensity of cathode rays that gave a brilliant light with Nos. 2 and 4, Nos. 1 and 3 were found to give practically no light, becoming barely red hot, while, as before, No 4 was found to give slightly more light than No 2, and to heat up more rapidly and cool more rapidly than the latter.

These experiments show that thoria and ceria, both alone and mixed, behave quite differently when heated by cathode ray bombardment than when heated in a Bunsen flame In the latter, 99 per cent. thoria plus 1 per cent. ceria gives many times as much light as pure thoria alone, while, when incandesced by cathode rays of equal intensity, the difference, though in a similar direction, is exceedingly small. Again, in the flame pure ceria gives just about the same amount of light as pure thoria, while with a given intensity of cathode ray bombardment thoria gives a brilliant light, while ceria gives practically none.

In arriving at any finally satisfactory theory of the luminescent properties of the rare earths, these results with cathode rays, which differ materially from those obtained by other methods of heating, will require to be taken into account.

I am indebted to the courtesy of the Welsbach Incandescent Gas Light Company for the samples of the rare earths with which the above investigations were made; also to the assistance of Mr. J. C. M. Stanton and Mr. H. Tyson Wolff in carrying out the experiments.

"On the Electrical Conductivity of Flames containing Salt Vapours." By HAROLD A. WILSON, B.Sc. (Lond. and Vic.), 1851 Exhibition Scholar. Communicated by Professor J. J. THOMSON, F.R.S. Received March 10,—Read April 27, 1899.

(Abstract.)

The experiments described in this paper were undertaken with the object of following up the analogy between the conductivity of salt vapours and that of Röntgenised gases, and especially of getting some information about the velocities of the ions in the flame itself.

They are to some extent a continuation of the research of which an abstract has already been published in the 'Proceedings of the Royal Society.'*

The paper is divided into the following sections:—

(1) Description of the apparatus for producing the flame.
(2) The relation between the current and E.M.F in the flame.
(3) The fall of potential between the electrodes.
(4) The ionisation of the salt vapour
(5) The relative velocities of the ions in the flame.
(6) The relative velocities of the ions in hot air.
(7) Conclusion.

The apparatus used for producing the flame was similar in principle to that used in the investigation referred to above. Carefully regulated supplies of coal gas and air were mixed together along with spray of a salt solution, and the mixture burnt from a brass tube 0·7 cm. in diameter.

The flame thus obtained was steady, and measurements of its conductivity, when a particular salt solution was sprayed, did not differ more than 1 or 2 per cent. on different days. The height of the inner sharply defined green cone was 1·5 cm., and that of the outer cone 7·5 cm.

The current between two gauzes of platinum wire, each 14 cm. in diameter, and placed horizontally one above the other in the flame, was measured for E.M.F.'s up to 800 volts, and with various distances between the gauzes.

The current with a large E.M.F. was found to be independent of the distance between the electrodes when the upper electrode or gauze was positively charged, provided that the distance between the electrodes was not so great that the upper one was in the cooler parts of the flame near its point. When the upper gauze was comparatively

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[*] "The Electrical Conductivity and Luminosity of Flames containing Vaporised Salts," by Arthur Smithells, H. M. Dawson, and H. A. Wilson, 'Roy. Soc. Proc.,' vol. 64, p. 142.

cool the current was much smaller, but if the upper gauze was kept hot by passing a current through it, then the current with a large E.M.F. was independent of the distance between the electrodes, even when the upper electrode was above the point of the flame

If both of the electrodes were hot, then the current, as the E.M.F. was increased, attained a nearly constant value. Cooling the positive electrode by raising it in the flame caused the current to increase towards this saturation value much more slowly than before, while cooling the negative electrode, the positive one being hot, caused the current to show no sign of arriving at a maximum value. The current was much greater when the negative electrode was hot, and the positive electrode cool, than when the negative electrode was cool, and the positive one hot

The fall of potential in the flame between the gauzes was examined by putting in an insulated platinum wire, and finding the potential it took up. When both the electrodes were hot, the fall of potential closely resembled that observed in gases at low pressures. That is to say, near each electrode there was a comparatively sudden fall of potential much greater near the negative electrode than near the positive, with a small and nearly uniform gradient in between. If either of the electrodes was cooled, then the fall of potential near that electrode became much greater, and often was nearly equal to the total drop of potential between the electrodes This effect was usually much more marked in the case of cooling the negative electrode than with the positive electrode.

If the positive electrode was uppermost and somewhat cool, then with small E.M.F.'s practically all the potential fall occurred near to the positive electrode; but if the E.M.F. was sufficiently increased, then a drop of potential appeared at the negative electrode, and with a still greater E.M.F. this became greater than that at the positive electrode, as it is in gases at low pressures.

Some of the results obtained pointed to the conclusion that nearly all the ionisation of the salt vapour takes place at the surfaces of the glowing electrodes, and not throughout the volume of the flame. A variety of experiments were tried to test this view, all of which confirmed its correctness. Thus, with two platinum foil electrodes opposite one another in the flame, no increase in the current between them occurred when a bead of salt was put between them, so that the salt vapour from it passed between them without touching either electrode. If the vapour came in contact with the negative electrode, there was a great increase in the current, and a considerable but smaller increase when it came in contact with the positive electrode.

The relative velocities of the ions of alkali metal salts in the flame were estimated by finding the potential gradient necessary to make the ions travel down the flame against the upward current of gases.

This was done by putting a bead of salt between the two gauze electrodes, and finding what E.M.F. was necessary to produce an increase in the current between the electrodes when the bead was put in.

The potential gradient corresponding to this least E.M.F. was then determined. In this way it was found that the positive ions of salts of Li, Na, K, Rb, and Cs, all have nearly the same velocity in the flame, whilst the negative ions of various salts of these metals also have equal velocities which are about seventeen times as great as the velocities of the positive ions.

The velocity of the positive ions was estimated to be about 60 cm. per second for one volt a cm., and that of the negative ions was about 1000 cm. per second.

The relative velocities of the ions of various salts was also determined in a current of air at about 1000° C., which was obtained by passing the air through a platinum tube 1·3 cm. in diameter, and 50 cm. long, heated in a gas-tube furnace. The method used was exactly analogous to that used in the flame. The ions could be divided into three classes, in each of which all the ions had equal velocities, viz.:—

	Velocity.
1. Negative ions of salts of Li, Na, K, Rb, Cs, Ca, Sr, and Ba	26·0 cm.-sec.
2. Positive ions of salts of Li, Na, K, Rb, and Cs	7·2 „
3. Positive ions of salts of Ca, Sr, and Ba . . .	3·8 „

It thus appears that those ions which in solutions carry equal charges have equal velocities in the gaseous state. This points to the conclusion that the velocity of a gaseous ion in a given medium depends only on its charge. The velocities are less than those calculated for ions consisting of one atom, so that each ion appears to be a cluster of atoms. If we regard this cluster as held together by the charge on it, then it is reasonable to suppose that the size of the cluster will be determined by the charge. Hence those ions having equal charges will be of equal sizes, and consequently of equal masses, since the atoms forming the cluster probably come from the medium rather than from the small quantity of salt present. Consequently they all have the same velocity under similar conditions.

The two main results arrived at in this paper, viz., that the ionisation of the salt vapour in the flame takes place only at the surfaces of the glowing electrodes, and that the velocity of the negative ions in the flame is very much greater than the corresponding velocity of the positive ions, enable the phenomena of unipolar conduction to be very easily explained. For example, if one electrode is much hotter than the other, then if the hot electrode is negative, it will give off negative ions very freely, and there will be a large current; but if the hot electrode is positive, then the small velocity of the positive ions is not favourable to their being dragged away from the electrode before they can recombine, so that the current is very small unless a very great E.M.F. is applied.

"On a Quartz-thread Gravity Balance." By RICHARD THRELFALL, lately Professor of Physics in the University of Sydney, and JAMES ARTHUR POLLOCK, lately Demonstrator of Physics in the University of Sydney Communicated by Professor J. J. THOMSON, F R.S. Received April 11—Read April 27, 1899.

(Abstract.)

The first part of the paper contains an account of the instrument in its present form, an account of the investigations leading up to the form adopted being relegated to an appendix.

The principle of construction is as follows:—A quartz thread (which requires to be prepared with much care) is stretched horizontally between two supports, to which it is soldered At one end the point of attachment is the centre of a spring of peculiar construction, designed so as to be capable of displacement in the direction of the thread, but incapable of any transverse motion or vibration

At the other end the thread is attached to the axle of a vernier arm moving over a sextant arc, by turning the axle the thread may be more or less twisted, the amount of twist being ascertained in terms of the divisions of the sextant arc

Midway between the two supports the thread is soldered to a short length of fine brass wire, which is adjusted so that the centre of gravity of the wire does not lie immediately above or below the thread, but at some distance from it The wire forming the "lever" is then rotated about the thread as axis in such a manner that the two halves of the thread are twisted through about three whole turns, and the torsion of the thread is then of such a value that the lever assumes a horizontal position. This adjustment is made by weighting the lever with a small speck of fusible metal. The "balance," which determines the position of the lever with respect to the horizontal plane through the thread, is composed of the earth's gravitational force on the one hand, and the forces of resilience of the twisted thread on the other. Were gravitational force to increase, the centre of gravity of the lever would fall, the end of the lever would move out of its sighted position, and the thread would have to be slightly twisted by the vernier axle in order to bring the lever back to its sighted position.

Differences in the gravitational intensity at different stations are expressed in terms of the amount by which one end of the thread has to be twisted or untwisted to bring the lever to its sighted position.

In carrying out the construction of the instrument on the principle thus explained the following conditions have to be fulfilled:—

The instrument must be portable, and must be able to withstand the rough usage inseparable from travelling, without being put out of adjustment. It must have a sensitiveness of at least one part in 100,000 of the value of "g," *i.e.*, a change in the value of "g" amounting to one part in 100,000 must be shown by the balance.

These conditions have been satisfied in the following manner:—The thread supports form part of a girder mechanism which is itself contained in a thermally insulated tube. During transport the lever is arrested by a mechanism which clamps it with a definite pressure in a definite position. The end of the lever is observed by a microscope which is always brought into the same relative position with respect to the horizontal plane through the thread by means of sensitive striding levels. It is shown as a consequence of the mechanical conditions that the lever will be in unstable equilibrium when its centre of gravity rises above the horizontal plane through the thread by about 3°. Consequently the accuracy with which the lever can be brought to its sighted position is very great, for the position selected as the sighted position is within a small fraction of a degree of the position of instability.

As it is necessary either to keep the balance in an atmosphere of constant density or to correct the observations for changes in the barometrical pressure, the former course was decided upon, and consequently the instrument is contained in an air-tight space. This involves working the vernier axle through a stuffing box which must be practically frictionless, a condition satisfied by a sort of mercury sealing.

The difficulty in making the apparatus arises from the fact that quartz fibres, though infinitely better than any other material, are not really sufficiently perfect in their elastic properties for the present purpose, and it is only by a judicious balancing of advantages that it is possible to arrive at the necessary sensitiveness. Even after two years' twisting the thread of the instrument still undergoes a continual, though small, viscous deformation, this, however, becomes sensibly constant, and can be allowed for.

A further complication arises from the fact that as the temperature rises the quartz becomes stiffer, so that at a given station the circle readings are a function of the temperature. We have found that the relation between the circle reading and the platinum temperature is a linear one at ordinary temperatures.

An essential feature of the apparatus, therefore, is a platinum wire thermometer placed alongside the thread.

The following statement shows concisely the effect on a determination of gravity of the various observational errors which are possible. The instrument of course only refers differences of gravity to a known difference. The results are expressed in round numbers, gravity being taken at its value in the latitude of Sydney.

Our temperature observations may be inconsistent by at most one-hundredth of a centigrade degree; this would correspond to an uncertainty of one part in 700,000 in the value of "g."

The accuracy with which the microscope can be set on the lever is much greater than the accuracy of reading the sextant arc. If our estimate of the latter is wrong by 5″ the resulting value of "g" is affected to the extent of one part in 1,300,000.

The errors of levelling may amount to one part in 700,000 in the value of "g." This gives a possible maximum uncertainty of one part in 300,000 in the value of "g" The daily rate of the instrument does not introduce an uncertainty of anything like the amounts mentioned above, and can in any case be eliminated by observing alternately at two stations, the difference in the value of gravity between them being the subject of observation

Observations.—Two observers are required, one for the balance and one at the thermometer resistance box.

It is only possible to observe with sufficient accuracy when the temperature is nearly steady; we always observe therefore at a time when the temperature passes through a maximum or a minimum value. With the instrument as constructed of various metals it is also necessary to avoid observing too soon after any great and sudden variation of temperature.

Journeys—We have travelled with the balance from Sydney to Melbourne by train, from Melbourne to Hobart by steamer, from Hobart to Launceston (in Tasmania) by train, back to Melbourne by steamer, and to Sydney by train. We have also made many less extended journeys in New South Wales, having travelled over more than 6000 miles with the instrument.

Most of these journeys led to our making improvements in the instrument, and therefore are not to be regarded as forming surveys.

If, however, a consistency of one part in 50,000 in the value of "g" be considered satisfactory, then the Tasmanian stations may be considered as surveyed, and the values assigned to gravity at these stations to be referred to the Melbourne-Sydney difference. Since this journey was undertaken the instrument has been so much improved in detail that we do not discuss its results from a gravitational point of view.

We have, however, made three test journeys between Sydney and Hornsby in New South Wales under proper conditions, and the result of these observations shows that at Hornsby the thread has to be untwisted at one end by the following amounts as referred to the reading at Sydney:—

Journey 1. Mean of Sydney—Hornsby and Hornsby—Sydney. Difference 18·5 sextant minutes.

Journey 2. Mean of Sydney—Hornsby and Hornsby—Sydney. Difference 18·1 sextant minutes.

Journey 3.—Mean of Sydney—Hornsby and Hornsby—Sydney. Difference 18 1 sextant minutes.

The maximum difference is thus 0·4 sextant minute, and corresponds to an uncertainty in the value to be assigned to the acceleration of gravity at Hornsby as compared with that at Sydney taken as known of one part in 500,000. This we believe to fairly represent the accuracy attainable by the instrument in actual field work. It is about double of the outside accuracy attainable by invariable pendulums, not connected by telegraph, and the observation takes about half an hour, but the time depends on the time required for the temperature to become steady The observations quoted took about three hours each. Packing and unpacking takes about an hour and a half, and the actual observing about five minutes, but the temperature must be watched to the maximum or minimum before the observations begin.

The weight of the instrument and of appliances taken directly from the laboratory and packed in strong boxes is 226 lbs , by making special appliances with a view to lightness this weight might be reduced to one-half.

The paper is illustrated by working drawings, &c.

"Data for the Problem of Evolution in Man. I. A First Study of the Variability and Correlation of the Hand." By Miss M. A. Whiteley, B Sc., and Karl Pearson, F.R.S. Received April 6,—Read April 27, 1899.

1 In a more purely theoretical discussion of the influence of natural selection on the variability and correlation of species, which one of the present writers hopes shortly to publish, a number of theorems are proved which it is desirable to illustrate numerically. But the quantitative measures of the variability and correlation hitherto published are comparatively few in number, especially when, as in the present case, we desire to have their values for a number of local races of the same species. When we have once realised that neither variability nor correlation are constant for local races but are modified in a determinate manner by natural selection, and further that their differences are the sure key to the problem of how selection has differentiated local races, then the importance of putting on record all the quantitative measures we can possibly ascertain of variability and correlation becomes apparent. For some five years past various members of the

Department of Applied Mathematics in University College, London, have, so far as their other work allowed, been collecting and reducing data concerning the variability and correlation of different organs and characters in man. So far as variability is concerned, 160 cases of organs in divers races of man were worked out by Miss Alice Lee, Mr. G. U. Yule, and one of the present writers some years ago,* and since then the more laborious task of measuring the correlation of characters and organs in man has been going steadily forward, until at the present time a considerable mass of material is reduced and ready for publication The present series of short papers is intended to cover this ground. It will simply state the numerical results reached and any obvious conclusions to be drawn from them, leaving to a later date the consideration of the material as a whole, and in particular its bearing on the general problem of evolution and the relationship of local races of man.

2. This first study deals only with one character of the hand in one sex and one race. A wider range of material on the skeleton of the hand in another local race is already being dealt with. But while the correlation of the anatomically simple parts of the hand is of very great importance, it does not follow that the complex members of the living hand may not be equally, or even more, significant when we have to deal with fitness for the struggle for existence. So far as we have been able to ascertain, although much has been written as to the fitness of the hand for its tasks, no attempt has ever been made to ascertain quantitatively the degree of correlation of its parts.† Hence our first object was to get some idea of the correlation of the parts of the hand from an easily measured and in practice important part Is the hand as highly correlated as the long bones, or as loosely correlated as the parts of the skull, or does it occupy some intermediate position like that of strength to stature? We accordingly selected as an easily measured but still important character the first joint of the fingers. The measurement therefore covers, besides the fleshy parts, the head of the metacarpal bone together with the proximal phalange. It is thus not anatomically simple, but it probably has much importance for the fitness of the hand, and is a measurement which with a little care can be made with considerable accuracy. Our measurements were taken with a small boxwood spanner graduated to 1/10 inch, and provided

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* A diagram was exhibited at a soirée of the Royal Society three years ago, and we shall be glad to send a photograph of that diagram to any one working at the problem of variation. The data without the diagram are published in a paper on "Variation in Man and Woman," 'The Chances of Death,' vol. 1, pp. 256—277.

† Here, as in other cases, both zoologists and anatomists have since the days of Cuvier, talked a good deal about correlation, but would even to-day be unable to reconstruct, with anything like *quantitative* accuracy, a skeleton from a long bone, a hand from a finger-joint, or a skull from a fragment.

with a vernier, so that the readings could be nominally made to 1/100 inch. Both the hands of 551 women were measured. At first it was proposed to include only those of more than 20 years of age, but no sensible difference was found for the means of those between 18 and 20, and accordingly some sixty or more between these years were included in the final results. While more than a moiety of the measurements and nearly all the laborious arithmetical reductions were made by one of us, Miss M. A. Whiteley, we owe measurements on the students of University, Girton, Newnham, and Westfield Colleges to the energetic assistance of Miss Dorothy Marshall, B.Sc., and a further ninety sets, principally from the students of Bedford College, to Miss Edith Humphrey, B.Sc., to both of whom we wish to acknowledge our great indebtedness.

In the tabulation of results the grouping was done to 1/20 inch, and the means, standard deviations, coefficients of variation, and coefficients of correlation, together with their probable errors, calculated by the processes and formulæ already fully described in papers of the series "Mathematical Contributions to the Theory of Evolution," by one of the present writers. Pianists were specially noted on the data cards, but their numbers did not seem sufficiently large to justify any conclusions as to the effect of use on variability and correlation—a subject which deserves very careful and special investigation.*

The following notation is used :—

Ri = first joint of right-hand index finger.
Rii = „ „ middle „
Riii = „ „ ring „
Riv = „ „ little „
Li = „ left-hand index „
Lii = „ „ middle „
Liii = „ „ ring „
Liv = „ „ little „

3. *Relative Size of the Hands.*—Turning first to the absolute dimensions of these joints we have, the measurements being in inches :—

Table I.—Lengths of First Joints of Fingers.

	R	L.
i.	2·2482 ± 0·0030	2·2252 ± 0·0031
ii.	2·3879 ± 0·0033	2·3667 ± 0·0033
iii.	2·2108 ± 0·0031	2·1878 ± 0·0031
iv.	1·8427 ± 0·0028	1·8197 ± 0·0028

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* What effect may particular trades or forms of exercise have in modifying the variability of the limbs used and their correlation to other limbs? The relative importance of use and of selection in determining the current values of variability and correlation will one day require very careful investigation.

We conclude at once that these joints in the right hand are very sensibly larger than in the left. In every case there is a difference of about 0·02, and this is many times larger than the probable error of the difference, *i.e.*, $\sqrt{2} \times 0{\cdot}003$ about.

We might, therefore, conclude that the right hand is larger than the left. This conclusion is directly opposed to that of W. Pfitzner,[*] he asserts that there is no quantitative difference between right and left for the simple anatomical parts of the hand skeleton. His own measurements, however, really do show such a sensible difference for the *first* phalange. All then we assert at present is that the first joint and the first phalange are larger in the right than in the left hand of women. We prefer to state no more sweeping view at present as to other parts of the hand, however strong our private opinion may be.

4. *Variability of the Hand.*—The following are the numerical results reached:—

Table II

	Standard deviation.	Coefficient of variation.
R i .	0·1055 ± 0·0021	4·6945 ± 0·0954
R ii .	0·1133 ± 0·0023	4·7432 ± 0·0964
R iii . ..	0·1091 ± 0·0022	4·9345 ± 0·0100
R iv	0·0986 ± 0·0020	5·3537 ± 0·0109
L i . ..	0·1088 ± 0·0022	4·8917 ± 0·0994
L ii	0·1137 ± 0·0023	4·8033 ± 0·0976
L iii . . .	0·1082 ± 0·0022	4·9481 ± 0·0101
L iv	0·0975 ± 0·0020	5·3614 ± 0·0109

If we were to judge by *absolute* variations the index and middle fingers of the right hand are less, the ring and little fingers more variable than those of the left hand But if we use the more reasonable coefficient of variation, we see that all the first joints for the left hand are more variable than the corresponding joints for the right hand, and this is precisely what we might expect if there be greater adaptation by selection, or by use of the right hand. The greater the selection, the less the variability.

In the left hand the relative order of variability (as measured by the coefficient of variation) is that of the relative size of the fingers; in the right hand this is slightly modified.[†] The work has been care-least correlation, and the pair of middle fingers higher correlation than the pair of ring fingers.

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[*] Dr. Gustav Schwalbe's 'Morphologische Arbeiten'; W. Pfitzner, 'Das Menschliche Extremitätenskelet,' Bd. I, pp. 21—35, and Bd. II, pp. 99—106, 1892 and 1893.

[†] The divergence is not one on which real stress can be laid considering the probable error of the coefficient of variation. The hand confirms what we have already learnt from the long bones ('Roy. Soc. Proc.,' vol. 61, pp 347—348), that 5 per cent. closely measures the variability of the chief parts of the human body.

Dr. Warren* has been the first to consider the correlation of corresponding right and left parts. He gives for ♀ series of Naqada bones —

R and L femur	0 9618 ± 0 0045
R and L tibia .	0·9505 ± 0·0047
R and L humerus	0·9643 ± 0·0047
R and L radius . .	0 9322 ± 0·0124

Hence we are compelled to conclude that the correlation between corresponding long bones (with the possible exception of that of the radii, which is within the probable error of the value for the middle fingers) is greater than that between corresponding parts of the two hands.

6. *Index Correlations*.—One of the present writers has previously expressed doubts of the validity of using index correlations as a measure of organic correlation.† At the same time it may not be without value to put on record the correlations between the finger joints expressed in terms of the first little finger joint as unit.

There are two methods of obtaining index correlations, either directly by forming the actual ratios and then grouping them in correlation tables, or indirectly from the variations and correlations of the absolute quantities by means of the formulæ given in the memoir cited in the footnote. The latter is by far the easier process, but it neglects what are usually small quantities of the third order. In order to justify the use of the latter method, the values of the constants for $i_{14} = \text{R i}/\text{R iv}$ and $i_{24} = \text{R ii}/\text{R iv}$ were found by both methods. They gave the following results, Σ_{14}, Σ_{24} being the standard deviations of the indices, V_{14}, V_{24} the coefficients of variation, and ρ the coefficient of correlation.

Table V.

	Directly. By correlation table.	Indirectly. By formulæ.	Difference.
i_{14}	1·2216	1·2210	+0·0006
i_{24}	1·2970	1·2968	+0·0002
Σ_{14}	0·0368	0·0379	−0·0011
Σ_{24}	0·0389	0·0395	−0 0006
V_{14}	3·0097	3·1013	−0 0916
V_{24}	3·0034	3·0487	−0·0453
ρ	0·7388	0 7631	−0·0243

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[*] 'Phil. Trans.,' B, vol. 189, p. 178.

[†] 'Roy. Soc. Proc.,' vol. 60, pp. 489—498.

It will be seen at once that the means and standard deviations obtained by the two methods are very close, but that in the coefficients of variation and correlation there may be a difference of some 3 per cent. Sensible as this is, its amount did not seem to justify the immense additional labour of index correlation tables—until at any rate the biologists have shown what possible use can be made of index correlations for *organic* relationship.

The following results were obtained —

Table VI

Index	Mean value	Standard deviation.
R i/R iv	1·2210	0·03787
R ii/R iv	1·2068	0·03954
R iii/R iv	1·2004	0·03270
L i/L iv	1·2238	0·03799
L ii/L iv	1·3016	0·04001
L iii/L iv	1·2030	0·03186

It would thus appear that the indices for the left hand are all larger than for the right, or the index, middle and ring fingers relatively larger with respect to the little finger in the left than the right hand. On the whole the variability of the right hand still appears less than that of the left, *i.e.*, two cases against one.

Turning to correlation, the following values were found —

Table VII.—Total Correlations of Indices

	Ri/Riv	Rii/Riv	Riii/Riv	Li/Liv	Lii/Liv	Liii/Liv.	
Ri/Riv	1	0·7631	0·6632	1	0·7774	0·6587	Li/Liv
Rii/Riv	0·7631	1	0·7310	0·7774	1	0·7590	Lii/Liv
Riii/Riv	0·6632	0·7310	1	0·6587	0·7590	1	Liii/Liv

Here, but not so decisively as in the case of absolute magnitudes, the left hand exhibits higher correlation. This higher correlation becomes absolutely decisive, however, if we consider the spurious correlations given below.

Table VIII.—Spurious Correlations of Indices.

	Ri/Riv.	Rii/Riv.	Riii/Riv	Li/Liv.	Lii/Liv.	Liii/Liv.	
Ri/Riv	1	0·5628	0·5529	1	0·5502	0·5429	Li/Liv
Rii/Riv	0·5628	1	0·5504	0·5502	1	0·5473	Lii/Liv
Riii/Riv	0·5529	0·5504	1	0·5429	0·5473	1	Liii/Liv

In every case the right hand exhibits more *spurious* correlation than the left, and our previous conclusion is thus thoroughly confirmed, the left hand exhibits higher organic correlation of its parts than the right. How is this to be explained ? It is all important that further researches should determine whether it is selection or use which differentiates the two hands. It would be hardly possible to find a sufficiently large group of left-handed persons to mark how far variation and correlation were modified; but measurements on the hands of children, of the educated and uneducated, and of workmen following particular trades might possibly throw light on the extent to which use modifies correlation.

We append the correlation tables giving the data upon which our numerical values are based

APPENDIX

Correlation Tables for First Finger Joints

I.—Index and Middle Fingers, Right Hand (R I and R II)

R I →	1·95 to 2·00.	2·00 to 2·05	2·05 to 2·10	2·10 to 2·15.	2·15 to 2·20.	2·20 to 2·25	2·25 to 2·30	2·30 to 2·35	2·35 to 2·40	2·40 to 2·45.	24·5 to 2·50	2·50 to 2·55	2·55 to 2·60	Totals.
2·00—2·05	0·5													0·5
2·05—2·10	0·5		2											2·5
2·10—2·15	2·5	2·5	2	0·5										7·5
2·15—2·20	1	6·5	5·5	3										16
2·20—2·25	0·5	3·5	12·5	15·5	1·5	1·5								35
2·25—2·30		0·5	6	14·5	22	5·5	2	0·5						50
2·30—2·35			1·5	12	38·75	31·5	10·75							94·5
2·35—2·40				2	16	39	28·5	6						91·5
2·40—2·45				0·5	1·25	18·25	47	26	3·5					96·5
2·45—2·50					2·5	2·25	10·5	32·25	17	1				66·5
2·50—2·55					0·5		1·25	16·75	20	11·5	1·5			51·5
2·55—2·60									7·75	9·75	2·5	0·5		20·5
2·60—2·65							0·5	0·5	1·25	6·25	4·5			13
2·65—2·70										2·5	1	0·5		4
2·70—2·75										0·5			1	1·5
Totals	5	13	29·5	48	82·5	98	100·5	82	49·5	31·5	9·5	1	1	551

I.—Index and Ring Fingers, Right Hand (R i and R iii)

R i →	1·95 to 2·00	2·00 to 2·05.	2·05 to 2·10	2·10 to 2·15.	2·15 to 2·20	2·20 to 2·25	2·25 to 2·30	2·30 to 2·35	2·35 to 2·40	2·40 to 2·45.	2·45 to 2·50	2·50 to 2·55	2·55 to 2·60	Totals.
1·85—1·90		0·5												0·5
1·90—1·95	1	0·75	1·75											3·5
1·95—2·00	3·25	1	1	0·25	0·5									6
2·00—2·05	0·75	8	10	5·75	2·5	1	1							29
2·05—2·10		2·75	10·75	19·25	9·25	1·5	0·25	0·25						44
2·10—2·15			3	14·5	35·5	16·25	4	0·25						73·5
2·15—2·20			1	6·75	26·5	42·75	22·25	4·25						103·5
2·20—2·25			1·5	1·5	7·25	29·25	39·5	22	2	0·5				103·5
2·25—2·30			0·5			7·25	28	26·25	8·5	2·5				73
2·30—2·35					0·5		4·5	20·25	25·75	6				57
2·35—2·40					0·5		1	7·75	10·5	10	2·25	0·5		32·5
2·40—2·45								1	1·5	10·25	3·25			16
2·45—2·50									1·25	2·25	3·5			7
2·50—2·55													1	1
2·55—2·60											0·5	0·5		1
Totals	5	13	29·5	48	82·5	98	100·5	82	49·5	31·5	9·5	1	1	551

III.—Index and Little Fingers, Right Hand (R i and R iv).

R i →	1·95 to 2·00	2·00 to 2·05	2·05 to 2·10	2·10 to 2·15	2·15 to 2·20	2·20 to 2·25	2·25 to 2·30	2·30 to 2·35	2·35 to 2·40	2·40 to 2·45	2·45 to 2·50	2·50 to 2·55	2·55 to 2·60	Totals
1·40—1·45	1													1
1·45—1·50														0
1·50—1·55														0
1·55—1·60	2													2
1·60—1·65	1	2·5	4·25	2·25										10
1·65—1·70		5	8·25	8·25	4·25	0·75	0·5							27
1·70—1·75	1	5·5	7·75	13·25	16·25	4·25	2·75	0·25						51
1·75—1·80			7·75	13·25	30·75	25·75	11·5	1·5						90·5
1·80—1·85			1·5	9	18	35·75	30·75	13·5	1					109·5
1·85—1·90				1	11·25	24·50	35·5	21·5	10·5	2·75				107
1·90—1·95				1	2	6	14·75	31·5	18·5	7·75	1·5			83
1·95—2·00							4·25	11·25	14	7·5	0·5			37·5
2·00—2·05						1	0·5	2·5	3·5	9·75	4·25	1		22·5
2·05—2·10									2	3·75	2·75			8·5
2·10—2·15											0·5		1	1·5
Totals....	5	13	29·5	48	82·5	98	100·5	82	49·5	31·5	9·5	1	1	551

IV —Middle and Ring Fingers, Right Hand (R ii and R iii)

R ii →	2·00 to 2·05	2·05 to 2·10	2·10 to 2·15	2·15 to 2·20	2·20 to 2·25	2·25 to 2·30	2·30 to 2·35	2·35 to 2·40	2·40 to 2·45	2·45 to 2·50	2·50 to 2·55	2·55 to 2·60	2·60 to 2·65	2·65 to 2·70	2·70 to 2·75	Totals
1·85—1·90			0·25	0·25												0·5
1·90—1·95			2·25	1·25												3·5
1·95—2·00	0·5	0·5	1·5	2·5	0·5	0·25	0·25									6
2·00—2·05		2	2·75	7·25	11·5	5·25	0·25									29
2·05—2·10			0·75	4·75	14·75	15·25	8·5									44
2·10—2·15					7	18·75	39	6·75	1·5	0·5						73·5
2·15—2·20					1·25	9·5	35	39·25	14·5	3·5	0·5					103·5
2·20—2·25						1·5	8·5	38	45	8	2·5					103·5
2·25—2·30						0·5	2	7·5	32·75	21·5	7·75	1				73
2·30—2·35									2·25	26·5	23	3·75	1·5			57
2·35—2·40							1		0·5	5·5	13·75	9·5	1·25	1		32·5
2·40—2·45											3	5	6·75	1	0·25	16
2·45—2·50											1	1·25	3·5	1	0·25	7
2·50—2·55															1	1
2·55—2·60														1		1
Totals .	0·5	2·5	7·5	16	35	51	94·5	91·5	96·5	65·5	51·5	20·5	13	4	1·6	551

V. Middle and Little Fingers, Right Hand (R ii and R iv.

R ii →	2·00 to 2·05	2·05 to 2·10	2·10 to 2·15	2·15 to 2·20	2·20 to 2·25	2·25 to 2·30	2·30 to 2·35	2·35 to 2·40	2·40 to 2·45	2·45 to 2·50	2·50 to 2·55	2·55 to 2·60	2·60 to 2·65	2·65 to 2·70	2·70 to 2·75	Totals.
1·40—1·45			1													1
1·45—1·50																0
1·50—1·55																0
1·55—1·60	0·5	0·5	1													2
1·60—1·65			2	4·25	2·75	1										10
1·65—1·70				6	12·5	3	4·5		0·5	0·5						27
1·70—1·75		0·75	3·25	5·25	8·75	18	11·25	2·75	1							51
1·75—1·80		0·25	0·25	0·5	8·25	15·5	37·25	20·5	7·5	0·5						90·5
1·80—1·85		1			2·75	9·5	24·0	37	27·25	5	2	1				109·5
1·85—1·90						3	15·75	22·25	34·75	19·75	11	0·5				107
1·90—1·95						1	1·75	7·5	22·75	26	16·25	5·5	1·25	1		83
1·95—2·00								0·5	2·75	12·5	11·25	5·5	4·5	0·5		37·5
2·00—2·05								1		1·25	8	5·75	4·5	1·75	0·25	22·5
2·05—2·10											3	2	2·5	0·75	0·25	8·5
2·10—2·15												0·25	0·25		1	1·5
Totals....	0·5	2·5	7·5	16	35	51	94·5	91·5	96·5	65·5	51·5	20·5	13	4	1·5	551

VI. Ring and Little Fingers, Right Hand (R iii and R iv)

R iii →	1·85 to 1·90	1·90 to 1·95	1·95 to 2·00	2·00 to 2·05	2·05 to 2·10	2·10 to 2·15	2·15 to 2·20	2·20 to 2·25	2·25 to 2·30	2·30 to 2·35	2·35 to 2·40	2·40 to 2·45	2·45 to 2·50	2·50 to 2·55	2·55 to 2·60	Totals
1·40—1·45			1													1
1·45—1·50																0
1·50—1·55																0
1·55—1·60		1	1													2
1·60—1·65	0·5	2	2	3·5	2											10
1·65—1·70		0·5	0·5	9	11·5	4·5	1									27
1·70—1·75			1	13·25	16·25	15·25	3·5	1·75								51
1·75—1·80				1·75	13·5	28	37	9	1·25							90·5
1·80—1·85				1	0·75	21	35·75	35·75	12·5	0·75	2					109·5
1·85—1·90			0·5	0·5		4·75	21·75	38·25	29	10·75	1·5					107
1·90—1·95							4·5	18·25	23	26	10·25		1			83
1·95—2·00								0·5	6·25	18·5	6·75	4·75	0·75			37·5
2·00—2·05									1	1	10·25	7·25	2		1	22·5
2·05—2·10											1·75	4	2·75			8·5
2·10—2·15													0·5	1		1·5
Totals	0·5	3·5	6	29	44	73·5	103·5	103·5	73	57	32·5	16	7	1	1	551

VII.—Index and Middle Fingers, Left Hand (L i and L ii)

Li→	1·90 to 1·95	1·95 to 2·00	2·00 to 2·05	2·05 to 2·10	2·10 to 2·15	2·15 to 2·20	2·20 to 2·25	2·25 to 2·30	2·30 to 2·35	2·35 to 2·40	2·40 to 2·45	2·45 to 2·50	2·50 to 2·55	2·55 to 2·60	Totals
2·00—2·05															0
2·05—2·10	2	1	2												5
2·10—2·15		1	6	2											9
2·15—2·20	1	1	11·75	10·75	1	1									26·5
2·20—2·25			6·5	14·75	13·25	2									36·5
2·25—2·30			0·75	8	34·5	27·5	3·75								74·5
2·30—2·35				1·5	12·25	36·5	37·25	10							97·5
2·35—2·40				0·5	3·5	20·5	38·25	25·75	9	1					98·5
2·40—2·45						2·5	25·75	31·5	18	2·75					80·5
2·45—2·50								7·75	30·25	12	1·5				81·5
2·50—2·55								1	15·75	15·25	6·5	2·5			41
2·55—2·60								0·5	3	5·75	8	0·75	1		19
2·60—2·65										1·25	4·5	1·75			7·5
2·65—2·70											1·5	1			2·5
2·70—2·75											0·5		0·5	0·5	1·5
Totals	3	3	27	37·5	64·5	90	105	76·5	76	38	22·5	6	1·5	0·5	551

VIII.—Index and Ring Fingers, Left Hand (L i and L m).

L i →	1·90 to 1·95	1·95 to 2·00	2·00 to 2·05	2·05 to 2·10	2·10 to 2·15	2·15 to 2·20	2·20 to 2·25	2·25 to 2·30	2·30 to 2·35	2·35 to 2·40	2·40 to 2·45	2·45 to 2·50	2·50 to 2·55	2·55 to 2·60	Totals.
1·85—1·90		0·5													0·5
1·90—1·95	1	1·5	2	1	0·5	0·5									6·5
1·95—2·00	1	0·75	5·75	3·5											11
2·00—2·05		0·25	15·25	12·5	9·5	1·5									39
2·05—2·10			3·75	13·5	24·5	8·75	2·25	0·75							53·5
2·10—2·15			0·25	5·5	23·5	38	17·75	6							91
2·15—2·20	1			1·5	6·5	33·25	39·5	22·75	4·5	2					111
2·20—2·25						8	39	27·25	17·5	0·25					92
2·25—2·30							5·5	14·5	26	10	1·5	1			58·5
2·30—2·35							1	3·75	21	15·75	5	1			47·5
2·35—2·40								1·25	6·75	6	7	1			22
2·40—2·45								0·25	0·25	3·75	7·75	2·5	0·5		15
2·45—2·50										0·25	1·25	0·5	1	0·5	3·5
Totals	3	3	27	37·5	64·5	90	105	76·5	76	38	22·5	6	1·5	0·5	551

IX.—Index and Little Fingers, Left Hand (L i and L iv).

L i →	1·90 to 1·95	1·95 to 2·00	2·00 to 2·05	2·05 to 2·10	2·10 to 2·15	2·15 to 2·20	2·20 to 2·25	2·25 to 2·30	2·30 to 2·35	2·35 to 2·40	2·40 to 2·45	2·45 to 2·50	2·50 to 2·55	2·55 to 2·60	Totals
1·50—1·55		1	1	1											3
1·55—1·60	1	1	1·5	0·5											4
1·60—1·65	0·5	0·5	5·5	6·5	4	0·5									17·5
1·65—1·70	1	0·5	9·75	10·25	8	3·5	1								34
1·70—1·75	0·5		5·25	9·75	21·25	14	7·25	2	1						61
1·75—1·80			2·5	6	21	35·75	31·5	10·25	0·5						107·5
1·80—1·85			1·5	3	7·75	29·25	41·75	27	13	4·25					127·5
1·85—1·90				0·5	1·5	6·5	18	24·5	26	6·5	2·5				86
1·90—1·95					1	0·5	5·5	10·5	26	12·5	5	1			62
1·95—2·00								2·25	6·75	12·5	6·5	1·5			29·5
2·00—2·05									2·75	1·75	6·5	2·5			13·5
2·05—2·10										0·5	1·5	1	1·5	0·5	5
2·10—2·15											0·5				0·5
Totals	3	3	27	37·5	64·5	90	105	76·5	76	38	22·5	6	1·5	0·5	551

X.—Middle and Ring Fingers, Left Hand (L ii and L iii)

L ii →	2·05 to 2·10	2·10 to 2·15	2·15 to 2·20	2·20 to 2·25	2·25 to 2·30	2·30 to 2·35	2·35 to 2·40	2·40 to 2·45	2·45 to 2·50	2·50 to 2·55	2·55 to 2·60	2·60 to 2·65	2·65 to 2·70	2·70 to 2·75	Totals.
1·85—1·90	0·5														0·5
1·90—1·95	1·5	3·5	0·5	1											6·5
1·95—2·00	1·5	1·5	5·75	2·25											11
2·00—2·05	1·5	4	15·5	13·75	4·25										39
2·05—2·10			3·75	14·75	26·25	6·5	2·25								53·5
2·10—2·15				4·75	32	42·25	10·25	1·75							91
2·15—2·20			1		11·75	39	42	15·25	1	1					111
2·20—2·25					0·25	6·75	39·5	40	4·75	0·75					92
2·25—2·30						3	4·5	19·25	21·75	8·75	1·25				58·5
2·30—2·35								4·25	19·5	18·75	4·25	0·75			47·5
2·35—2·40									4	8·75	5	3·25	0·5	0·5	22
2·40—2·45									0·5	3	7·75	2·25	1·5		15
2·45—2·50											0·75	1·25	0·5	1	3·5
Totals	5	9	26·5	36·5	74·5	97·5	98·5	80·5	51·5	41	19	7·5	2·5	1·5	551

XI.—Middle and Little Fingers, Left Hand (L ii and L iv).

L ii →	2·05 to 2·10	2·10 to 2·15	2·15 to 2·20	2·20 to 2·25	2·25 to 2·30	2·30 to 2·35	2·35 to 2·40	2·40 to 2·45	2·45 to 2·50	2·50 to 2·55	2·55 to 2·60	2·60 to 2·65	2·65 to 2·70	2·70 to 2·75	Totals
1·50—1·55	1	1	1												3
1·55—1·60	1	1·5	0·5	1											4
1·60—1·65	0·5	2·5	5·5	5	4										17·5
1·65—1·70	2·5	2	6	11·25	9·75	1·5	1								34
1·70—1·75		1	8·5	12·75	17·75	14·75	3·75	2·5							61
1·75—1·80			4	3·75	26·75	39·5	22·75	10·5	0·25						107·5
1·80—1·85		1	1	2·75	12·5	29·5	43·5	25·5	8·75	2	1				127·5
1·85—1·90					2·75	10·5	23	23	15·75	8·5	2·5				86
1·90—1·95					1	1·75	4·5	18·5	17·75	14·5	3	1			62
1·95—2·00								0·5	7·25	11·25	6·5	3	0·5	0·5	29·5
2·00—2·05									1·75	3·25	4	2·5	2		13·5
2·05—2·10										1·5	2	0·5		1	5
2·10—2·15												0·5			0·5
Totals	5	9	26·5	36·5	74·5	97·5	98·5	80·5	51·5	41	19	7·5	2·5	1·5	551

XII.—Ring Finger and Little Finger, Left Hand (L iii and L iv)

L iii →	1·85 to 1·90	1·90 to 1·95	1·95 to 2·00	2·00 to 2·05	2·05 to 2·10	2·10 to 2·15	2·15 to 2·20	2·20 to 2·25	2·25 to 2·30	2·30 to 2·35	2·35 to 2·40	2·40 to 2·45	2·45 to 2·50	Totals
1·50—1·55	0·5	1·5	1											3
1·55—1·60		3		1										4
1·60—1·65		2	4·5	7	3·5	0·5								17·5
1·65—1·70			4·5	14	12·5	2·5	0·5							34
1·70—1·75			1	10	20	20	9	1						61
1·75—1·80				4	12	41·75	37·25	11	1·5					107·5
1·80—1·85				3	5·25	21·5	43	42·75	8·5	3	0·5			127·5
1·85—1·90					0·25	3·75	19·25	26·75	24·75	9·25	2			86
1·90—1·95						1	2	10·5	22	21·5	2·5	2·5		62
1·95—2·00									1·75	11·75	12	3·5	0·5	29·5
2·00—2·05										2	4	6	1·5	13·5
2·05—2·10											1	2·5	1·5	5
2·10—2·15												0·5		0·5
Totals....	0·5	6·5	11	39	53·5	91	111	92	58·5	47·5	22	15	3·5	551

XIII.—Index Fingers, Right and Left Hands (R i and L i)

Ri→	1·95 to 2·00	2·00 to 2·05	2·05 to 2·10	2·10 to 2·15	2·15 to 2·20	2·20 to 2·25	2·25 to 1·30	2·30 to 2·35	2·35 to 2·40	2·40 to 2·45	2·45 to 2·50	2·50 to 2·55	2·55 to 2·60	Totals.
1·90—1·95	1	1	1											3
1·95—2·00	1	2												3
2·00—2·05	1·5	7·5	13	4		1								27
2·05—2·10	1·5	2·5	12	17	3·5	1								37·5
2·10—2·15			3·5	21	32	7	1							64·5
2·15—2·20				4·5	38·75	35·5	9·75	1·5						90
2·20—2·25				1·5	7·25	45·25	45·25	5·25	0·5					105
2·25—2·30					1	8·25	36·5	28·5	2·25					76·5
2·30—2·35							7	41·25	24·5	3·25				76
2·35—2·40							1	5·5	17·75	13·5	0·25			38
2·40—2·45									3·5	12	6·5	0·5		22·5
2·45—2·50									1	2·75	1·75	0·5		6
2·50—2·55											1		0·5	1·5
2·55—2·60													0·5	0·5
Totals	5	13	29·5	48	82·5	98	100·5	82	49·5	31·5	9·5	1	1	551

XIV.—Middle Fingers, Right and Left Hands (R ii and L ii).

R ii →	2·00 to 2·05	2·05 to 2·10	2·10 to 2·15	2·15 to 2·25	2·20 to 2·25	2·25 to 2·30	2·30 to 2·35	2·35 to 2·40	2·40 to 2·45	2·45 to 2·50	2·50 to 2·55	2·55 to 2·60	2·60 to 2·65	2·65 to 2·70	2·70 to 2·75	Totals
2·05—2·10	0·5	1	1·5	2												5
2·10—2·15			3·75	4·25	1											9
2·15—2·20		1·5	2·25	8·25	13	1·5										26·5
2·20—2·25				1·5	17·5	13	4·5									36·5
2·25—2·30					3·5	32·5	31·5	5·5	0·5	0·5	0·5					74·5
2·30—2·35						2	43·75	43	5·75	3						97·5
2·35—2·40						1·5	13·75	37·25	41	5						98·5
2·40—2·45						0·5	1	5·75	44·75	25·5	2·5		0·5			80·5
2·45—2·50									4·5	23·5	21·75	1·25	0·5			51·5
2·50—2·55										8	23·25	7·5	1·25	1		41
2·55—2·60											3·25	9·5	6	0·25		19
2·60—2·65											0·25	2·25	4·75	0·25		7·5
2·65—2·70														2·25	0·25	2·5
2·70—2·75														0·25	1·25	1·5
Totals....	0·5	2·5	7·5	16	35	51	94·5	91·5	96·5	65·5	51·5	20·5	13	4	1·5	551

XV. Ring Fingers, Right and Left Hands (R iii and L iii

R iii →	1 85 to 1 90	1·90 to 1·95	1 95 to 2 00	2·00 to 2·05	2 05 to 2 10	2·10 to 2 15	2·15 to 2·20	2·20 to 2·25	2 25 to 2·30	2·30 to 2 35	2 35 to 2·40	2·40 to 2·45	2 45 to 2·50	2·50 to 2 55	2 55 to 2 60	Totals.
1·85—1·90			0 5													0·5
1·90—1·95	0·5	3	1	1	1											6·5
1·95—2·00		0 5	1 5	6·5	2		0 5									11
2·00—2·05			2	16·25	17·25	2·5	1									39
2·05—2·10			0 5	4·75	18	24·5	5 75									53·5
2·10—2·15			0·5	0·5	4 25	40 5	38 75	5·75	0 75							91
2·15—2·20					1·5	6	48	43 75	10 25	0 25	1 25					111
2·20—2 25							9 5	48 5	31	2 25	0 75					92
2·25—2 30								5 25	26	23 75	3 5					58 5
2·30—2 35								0·25	5	27	12 75	2 5				47 5
2·35—2·40										2·25	11 25	4 5	4			22
2·40—2·45										1 5	3	8·25	1 75		0 5	15
2·45—2·50												0·75	1 25	1	0 5	3·5
Totals....	0·5	3·5	6	29	44	73 5	103·5	103 5	73	57	32 5	16	7	1	1	551

XVI.—Little Fingers, Right and Left Hands (R iv and L iv).

R iv →	1·40 to 1·45	1·45 to 1·50	1·50 to 1·55	1·55 to 1·60	1·60 to 1·65	1·65 to 1·70	1·70 to 1·75	1·75 to 1·80	1·80 to 1·85	1·85 to 1·90	1·90 to 1·95	1·95 to 2·00	2·00 to 2·05	2·05 to 2·10	2·10 to 2·15	Totals.
1·50—1·55	1			1	1											3
1·55—1·60				1	2 25	75										4
1·60—1·65					4·5	9 25	3·75									17·5
1·65—1·70					1·25	13	17	1·75	1							34
1·70—1·75					1	3	21·75	26 75	7·5	0 5	0 5					61
1·75—1·80							6	52·75	40·25	6	1·5	0·5	0 5			107·5
1·80—1·85						1	1 5	8 25	48·25	57·25	9	1·75	0 5			127·5
1·85—1·90							1	1	12 5	37 25	33·75	0·5				86
1·90—1·95										6	35 25	18·25	1	1·5		62
1·95—2·00											3	14 75	9·25	2 5		29·5
2·00—2·05												1·75	10	1 75		13·5
2·05—2·10													1·25	2·25	1·5	5
2·10—2·15														0·5		0·5
Totals....	1	0	0	2	10	27	51	90 5	109 5	107	83	37·5	22·5	8·5	1 5	551

XVII.—Ratios of Index and Middle Finger to Little Finger.

Right hand (Ri/Riv and Rii/Riv).

Ri/Riv →	1·075 to 1·100	1·100 to 1·125	1·125 to 1·150	1·150 to 1·175	1·175 to 1·200	1·200 to 1·225	1·225 to 1·250	1·250 to 1·275	1·275 to 1·300	1·300 to 1·325	1·325 to 1·350	1·350 to 1·375	1·375 to 1·400	Totals
1·125—1·150			1											1
1·150—1·175	0·5	0·5												1
1·175—1·200			0·25	0·25	0·5									1
1·200—1·225			1·25	5·25	2·5									9
1·225—1·250			0·75	16·75	24·25	4·75	0·5							47
1·250—1·275		2	1·25	16·25	35·75	28·25	9·5	1						94
1·275—1·300			1	2	22·5	63	39·5	5·5	1·5					135
1·300—1·325				3	11·5	38	58	26	4	1·5				142
1·325—1·350				1	1	9	32·5	32·5	8	2	1			87
1·350—1·375							6	8	7		1			22
1·375—1·400								2·75	2·5	2·25				7·5
1·400—1·425								1·75	0·5	0·25				2·5
1·425—1·450											0·5			0·5
1·450—1·475											0·5			0·5
1·475—1·500													1	1
Totals	0·5	2·5	5·5	44·5	98	143	146	77·5	23·5	6	3	0	1	551

May 4, 1899.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates recommended for election into the Society were read, as follows:—

Barrett, Professor William.
Booth, Charles, D Sc.
Bruce, David, Major R A M.C.
Fenton, Henry John Horstman, M.A.
Gamble, James Sykes, M.A.
Haddon, Professor Alfred Cort, M.A.
Head, Henry, M.D.
Hele-Shaw, Professor Henry Selby, M.Inst.C.E.
Morgan, Professor Conwy Lloyd, F G.S.
Reid, Clement, F.G.S.
Starling, Ernest Henry, M.D.
Tanner, Professor Henry William Lloyd, M.A.
Threlfall, Richard, M.A.
Tutton, Alfred E., B Sc.
Windle, Professor Bertram Coghill Allen, M.D.

The following Papers were read:—

I. "Photographic Researches on Phosphorescent Spectra." By Sir W. Crookes, F.R.S.

II. "On the Chemical Classification of the Stars." By Sir Norman Lockyer, K.C.B., F.R.S.

III. "On the Presence of two Vermiform Nuclei in the Fertilised Embryo-sac of *Lilium Martagon*." By Miss E. Sargant. Communicated by Dr. D. H. Scott, F.R S.

IV. "*Onygena equina* (Willd): a Horn-destroying Fungus." By Professor H. Marshall Ward, F.R.S.

V. "Impact with a Liquid Surface, studied by the aid of Instantaneous Photography. Paper II." By Professor Worthington, F.R.S., and R. C. Cole.

VI. "The external Features in the Development of *Lepidosiren paradoxa* (Fitz.)." By J. G. Kerr. Communicated by A. Sedgwick, F.R.S.

VII. "An Observation on Inheritance in Parthenogenesis." By Dr. E. Warren. Communicated by Professor Weldon, F.R.S.

VIII. "The Thermal Expansion of Pure Nickel and Cobalt." By A. E. TUTTON, B.Sc. Communicated by Professor TILDEN, F.R.S.

The Society adjourned over Ascension Day to Thursday, May 18.

"Impact with a Liquid Surface, studied by the aid of Instantaneous Photography. Paper II." By A. M. WORTHINGTON, M.A., F.R.S., and R. S. COLE, M.A. Received March 21,—Read May 4, 1899.

(Abstract.)

This paper is a continuation of a paper under a similar title, published in the 'Philosophical Transactions,' A, vol. 189, 1897.

It was there shown that between the splash of a rough and that of a polished sphere falling the same distance into water, there is a remarkable difference from the first moment of contact. The causes of this difference are now investigated.

The configuration of the water surface below the general level, when a rough sphere enters, is first studied by instantaneous photography, and the origin is traced of the bubble that follows in the wake of the sphere and of the emergent jet which follows its disappearance. The depression or crater formed round the entering sphere is surprisingly deep. This cavity segments, the lower part following as a bubble in the wake of the sphere, while the upper part fills up by the influx of surrounding water, which gathers velocity as it converges towards the axis of the disturbance, and so produces the upward spurt of the jet.

Experiments are described in which some idea of the actual displacements in the liquid has been obtained by letting the sphere descend between two vertical slowly ascending streams of minute bubbles liberated by electrolysis from two pointed electrodes.

It is found that with a gradual increase in the height of fall of a well-polished sphere, the splash changes in character, and that the sphere soon begins to take down air. But the height at which this is first noticeable is largely dependent on minute differences in the condition of the surface, and even on its temperature. It was further found that dropping a smooth sphere through a flame, under certain conditions, invariably alters entirely the course of the splash. This action of the flame is proved to be no action of electrical discharge, and reasons are given for attributing it to the burning off of fine dust which has collected on the surface during the fall.

The influence of dust was proved by dusting one side only of a polished sphere, a proceeding which always results in completely changing the character of the splash on the dusted side.

A satisfactory general explanation of all the phenomena is found in the view that with a smooth sphere, cohesion is operative in guiding the advancing edge of the liquid sheath which rises over and closely envelops the sphere. If the surface is not rigid (*e.g.*, is dusty), or is rough, then the momentum of the sheath carries it, once for all, away from the surface of the sphere, and the subsequent motion is quite different. The persistence of the remarkable radial ribs or flutings observable in the film that ensheathes a smooth entering sphere is completely explained by the assumption of a viscous drag spreading from the surface of the sphere outwards, and these flutings are always absent from any part of the sheath that has left the sphere. Their presence is an indication that there is no finite slip at the solid surface.

Experiments made with water mixed with glycerine show that, up to a certain point, the character of the disturbance is but slightly affected by large changes in viscosity. With pure glycerine, however, a thin film of water absorbed from the atmosphere equivalent to a layer $\frac{1}{20}$ mm. thick, was found completely to change the course of a splash, a striking proof of the importance of the initial motion in determining that which is to follow.

Experiments conducted *in vacuo* prove that the presence of the air has no noticeable influence on the early course of a splash, but that its pressure subsequently prevents cavitation of the liquid under what would otherwise be negative pressures.

The paper concludes with a reference to the remarkable similarity between the splash at the surface of a liquid and that caused at the surface of a hard-steel armour-plate by the impact of a projectile, and with the suggestion that the explanation may be found in the argument of Poynting,* which demands an increase of molecular mobility with increase of pressure.

"An Observation on Inheritance in Parthenogenesis." By ERNEST WARREN, D.Sc., University College, London. Communicated by Professor W. F. R. WELDON, F.R.S. Received March 22, —Read May 4, 1899.

On certain theoretical grounds it has been supposed by Weismann that offspring produced by parthenogenesis exhibit little or no variability. To determine how far this conclusion was warranted by fact, some measurements were made on *Daphnia magna* (Straus).†

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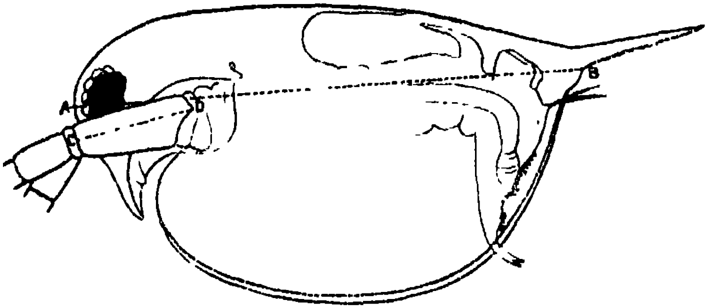
* Poynting, "Change of State, Solid-Liquid," 'Phil. Mag.,' July, 1881; see also two very important papers by Tresca on the "Flow of Solids," 'Proceedings of Institution of Mechanical Engineers,' June, 1867, and June, 1878.

† The measurements were made under the microscope with Zeiss's screw-micrometer.

The dimensions taken were :—

(1) The *total length of the body* measured along a line passing ventrally from the base of the spine and cutting the convex surface of the head opposite the middle of the compound eye (AB, see figure)



(2) The *length of the protopodite* of the 2nd antenna of the right side. The measurement was made on the posterior surface of the protopodite along a line parallel to the dorsal edge At the articulation with the head the exo-skeleton of the protopodite possesses a well defined point, which forms a good inner limit to the measurement (CD).

Since, under favourable conditions, these animals continue to grow throughout life, the second dimension was expressed in terms of the first, thus $\frac{\text{Length of R. protopodite}}{\text{Total length of body}} \times 1000$.

The mean of the relative length of the protopodite sinks as the animal grows, but between a body length of 2·4 mm. and 3·6 mm. (the total range of size) the change would not be large. I find that at the time of measurement the offspring were constantly somewhat smaller (0·4 to 0·5 mm.) than the parents, but as this applies to *all* the broods which were measured, the rise in the mean of the offspring would not affect the correlation surface.

From twenty-three Daphnia (themselves originating by parthenogenesis) broods were produced consisting of three to six individuals. The parents were measured, and the offspring were allowed to grow up. On measuring the offspring it was at once obvious that the children of the same brood exhibited very considerable variability.

In the following table (p. 156) the results of the measurements are displayed in a correlation surface.

The table illustrates the variability of children of the same parthenogenetic family, and we can further see, for example, that offspring with a parentage of 169·5 thousandths exhibited a range of variation 159·5—181·5 thousandths.

The following constants were calculated :—

Parentage. $\frac{\text{Length of R. protopodite}}{\text{Total length of body}} \times 1000$.		Correlation surface.														Weighted mothers.	Actual No. of mothers
159—160	1	..	..	..	..	..	..	..	..	..	..	2	1	2	..	5	1
161—162	2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	0	0
163—164	3	..	..	..	..	..	..	..	1	5	3	..	2	1	..	12	2
165—166	4	..	..	..	..	1	2	2	3	2	3	6	2	2	..	23	6
167—168	5	..	..	..	..	..	.	..	3	1	2	3	..	..	1	10	2
169—170	6	..	..	1	4	4	4	1	2	..	1	1	1	..	1	20	5
171—172	7	..	..	..	..	1	2	1	..	..	..	.	..	..	.	4	1
173 - 174	8	1	..	..	2	2	2	1	..	..	..	2	2	..	..	12	3
175—176	9	..	..	1	..	2	3	..	..	..	..	..	..	.	.	6	2
177—178	10	..	..	..	..	..	..	..	1	..	2	1	..	..	..	4	1
Offspring.		14	13	12	11	10	9	8	7	6	5	4	3	2	1		
		186—185	184—183	182—181	180—179	178—177	176—175	174—173	172—171	170—169	168—167	166—165	164—163	162—161	160—159	96	23
No. of Offspring.		1	0	2	6	10	13	5	10	8	11	15	8	5	2	96	

1. The standard deviation (S. D.) of the mothers weighted according to the number of offspring produced . .. . = 2 2208
2. The standard deviation of the offspring ... . = 2 9503
3. The standard deviation of array of offspring . . .. = 2·6104
4. The coefficient of correlation .... = 0·466 ± 0 0539
5. The coefficient of regression of offspring on mothers . . . . = 0 619 ± 0·0809

According to Mr. Galton's theory of ancestral heredity, a child, on the average, inherits 1/4th of any inherited character from either of its parents, 1/16th from any one of its grandparents, 1/64th from any one of its eight great grandparents, and so on

From a mathematical standpoint Professor Pearson* has examined Mr. Galton's theory, and he finds that if it be expressed in the form

$$\left(\tfrac{1}{4}\right)^n \frac{\text{S.D. of offspring}}{\text{S.D. of individual parent of the } n\text{th generation}}$$

the coefficients of correlation and regression between offspring and any generation of ancestors flow directly from it. Professor Pearson shows that the total regression of the progeny on the *mid-parent* of any generation is constant and is equal to 0 6, while the correlation and regression of an *individual parent* of the nth generation (supposing equal variability for all generations) = $0·6(\tfrac{1}{2})^n$ and the correlation of the *mid-parent* of the nth generation = $0\ 6\left(\frac{1}{\sqrt{2}}\right)^n$.

Hence the coefficients of correlation and regression of an individual parent of the 1st generation (*i.e.*, *father* or *mother*) = $0·6(\tfrac{1}{2})^1 = 0·3$, and the coefficient of correlation of *the mid-parent* = $0·6\left(\frac{1}{\sqrt{2}}\right)^1 = 0·424$, and the coefficient of regression, as we have just seen above, = 0·6.

Now, on comparing observation with theory, we see that the parthenogenetic mother appears to act like a mid-parent; the coefficients of correlation and regression being respectively 0·466 and 0·619. Further, we know $\frac{\text{S.D. of mid-parents}}{\text{S.D. of progeny}} = \frac{1}{\sqrt{2}} = 0·71$, and in the present case $\frac{\text{S.D. of parthenogenetic mothers}}{\text{S.D. of progeny}} = \frac{2·22}{2·95} = 0·75$.

Among my notes there are recorded the measurements of twenty-six grandchildren, the offspring of seven grandparents. With these the coefficients of correlation and regression were calculated. On account of the altogether insufficient number of individuals, the results were bound to be very uncertain, but they appear to favour the view that inheritance in parthenogenetic generations resembles that from mid-grandparent to grandchildren. The coefficient of correlation was 0·272 ± 0·12, and the coefficient of regression = 0·5 ± 0·2, while, according to theory, they should be 0·3 and 0·6 respectively.

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* 'Roy. Soc. Proc.,' vol. 62, pp. 386—412.

The evidence of these measurements cannot be said to be conclusive, and I am about to test the theory on some other parthenogenetic animal. If, however, this kind of inheritance be found to hold at all generally in parthenogenesis, it would be a fact of very considerable significance, and might conceivably give some insight into the physiological causes of heredity and variation.

"*Onygena equina* (Willd.): a Horn-destroying Fungus." By H. MARSHALL WARD, D.Sc., F.R.S., Professor of Botany in the University of Cambridge. Received April 6,—Read May 4, 1899.

(Abstract.)

The genus *Onygena* comprises half a dozen species of fungi, all very imperfectly known, remarkable for their growth on feathers, hair, horn, hoofs, &c., on which their sporocarps appear as drum-stick shaped bodies 5—10 mm. high. A cow's horn, thoroughly infested with the mycelium of the present species, yielded material for the investigation, and the author has not only verified what little was known, but has been able to cultivate the fungus and trace its life-history, neither of which had been done before, and to supply some details of its action on the horn.

The principal new points concern the development of the sporophores, which arise as domed or club-shaped masses of hyphæ and stand up into the air covered with a glistening white powder. Closer investigation shows this to consist of chlamydospores, formed at the free ends of the up-growing hyphæ. Their details of structure and development are fully described, and their spore nature proved by culture in hanging drops. The germination, growth into mycelia, and peculiar biology of these hitherto unknown spores were followed in detail, and in some cases new crops of chlamydospores obtained direct in the cultures.

When the crop of chlamydospores on the outside of the young sporophore is exhausted, the hyphæ which bore the spores fuse to form the peridium clothing the head of the sporocarp, and peculiar changes begin in the internal hyphæ below.

Minute tufts or knots of claw-like filaments spring from the hyphæ forming the main mass of the fungus, push their way in between the latter, and so find room in the mesh-like cavities. Here the closely segmented claws form asci—they are the ascogenous hyphæ—and the

details of development of the asci, their nucleated contents, and the spores are determined. As the spores ripen, the asci, which are extremely evanescent, disappear, and in the ripe sporocarp only spores can be seen lying loose in the meshes of the gleba. The ascomycetous character of the fungus is thus put beyond question, though the peculiar behaviour of the developing ascogenous tufts at one time rendered it questionable whether the older views as to the relationships were not more probable

No one had hitherto been able to trace the germination of these ascospores—the only spores known previously—and De Bary expressly stated his failure to do it. The author finds that they require digesting in gastric juice, and so in nature they have to pass through the stomach of the animal By using artificial gastric juice, and employing glue and other products of hydrolysis of horn, the details of germination and growth into mycelia, capable of infecting horn, were traced step by step under the microscope and fully described

No trace of any morphological structure comparable to sexual organs could be discovered, though many points suggest the alliance of this fungus with Erysipheæ and Truffles.

The author also found that similar digestion promotes the germination of the chlamydospores, and in both cases has not only traced the germination step by step, but has made measurements of the growth of the mycelium, induced the formation of chlamydospores on the mycelium again, and by transferring vigorous young mycelia to thin shavings of horn has observed the infection of the latter.

It thus becomes evident that the spores of *Onygena* pass through the body of an animal in nature, and, as might be expected from this, extract of the animal's dung affords a suitable food medium to re-start the growth on horn. Probably the cattle lick the *Onygena* spores from their own or each other's hides, hoofs, horns, &c., and this may explain why the fungus is so rarely observed on the living animal: it is recorded from such in at least one case however.

Very little is known as to the constitution of horn, and some experiments have been made to try to answer the question—what changes the fungus brings about. The research has also obvious bearings on the question of the decomposition of hair, horn, feathers, hoofs, &c., used as manure in agriculture. Although a bacterial decomposition of hoof substance is known to the author, special investigation of the question showed that in the present case no symbiosis between bacteria and the *Onygena* exists.

For the details as to the literature, the discussion as to the systematic position of *Onygena*, the experimental cultures, growth measurements, and the histology, the reader is referred to the full paper, which is illustrated by plates and numerous drawings.

"The External Features in the Development of *Lepidosiren paradoxa*, Fitz. By J. GRAHAM KERR. Communicated by A. SEDGWICK, F.R.S. Received April 11,—Read May 4, 1899.

(Abstract.)

The paper opens with a short account of the habits of *Lepidosiren* as observed in the Gran Chaco. A description is then given of the external features in the development. The more important points in this may be summarised as follows.

The egg is very large, 6·5—7 mm in diameter. It is surrounded by a special capsule at first thick and almost jelly-like in appearance, later on (after fertilisation) thin and horny. Outside this was found in rare cases a thick jelly resembling that of the common frog's egg. The egg is without a trace of dark pigment. Segmentation is complete, resembling most nearly that of the egg of *Amia*, and leads to a condition with an upper hemisphere of small cells with large segmentation cavity, and a lower of large yolk cells. Gastrulation begins with the appearance of a row of depressions, or a continuous groove along about one-third of the whole extent of the margin between small and large cells. During its progress the small-celled portion spreads over the lower yolk cells by the addition to its margin of small cells split off from the yolk cells. As the groove referred to deepens into a slit to form the archenteron, it becomes gradually shorter, and the eventual complete blastopore is a crescentic slit only about a quarter of the length of the original groove. The medullary folds soon appear running forwards from the blastopore. There is no trace externally of a blastoporic or *protostomial* seam running along the back between the medullary folds. The folds are low and inconspicuous, and they are continued into one another behind the blastopore, which becomes the anus. There are only slight traces of overarching of the medullary folds to enclose a neural canal. During the later stages of intraoval development, the posterior end of the body becomes much more conspicuously folded off the yolk than the head end. The Lepidosiren hatches out as a tadpole-shaped larva, still completely devoid of dark pigment. Just about the time of hatching the cloacal opening closes temporarily. As the larva develops it becomes extraordinarily amphibian-like. It possesses large pinnate external or somatic gills, four on each side, corresponding to branchial arches I, II, III, and IV. A large cement organ is also present, which during its early stages is of the characteristic crescent shape so usual in the embryos of Anura. Pigment begins to appear about ten days after hatching—first in the retina, then over the dorsal surface, especially anteriorly. The larval condition lasts during the first six weeks after hatching. Towards the end of this period the cement organ undergoes atrophy. The somatic gills atrophy later. During the process of their doing so, the Lepidosiren passes through a condition in which the stumps persist evidently corresponding to that well known in the young Protopterus, the group of external gills with their common stalk having come by differential development to be situated immediately above the fore limb. After the close of the larval period the Lepidosirens become much darker in colour and more lively in their movements. Young were obtained from the nest up to a length of 60 mm. About this time the cornea begins to assume the white unhealthy appearance that it has in the adult. In the young of this size, small yellow spots appear, and in the young of 90 mm. these are conspicuous. Occasional yellow blotches persist in the young Lepidosiren of eighteen months, but in the adult they disappear.

The paper concludes with general remarks on the phenomena described. The segmentation approaches most closely that of Ganoids The shortening up of the invaginating groove is considered to illustrate a process which has taken place in phylogeny in the passage from the primitive holoblastic egg to the meroblastic condition. The continuity of the medullary folds behind the anus is adduced, together with the evidence accumulating of the prolongation of the blastopore along the floor of the medullary groove in other forms (Amphibia, Ceratodus, *e.g.*) as affording potent evidence in favour of the hypothesis which derives the Vertebrata from ancestral forms as primitive as the Cœlenterata, and possessing a nelongated mouth traversing the neural surface. The occurrence of external gills in the young of three so comparatively primitive groups of Vertebrata as Crossopterygians, Dipnoans, and Amphibians, their occurrence on four branchial arches in Lepidosiren, and on at least the hyoid arch in Crossopterygians, and the occurrence of a probable homologue on the mandibular arch in Urodela, are taken as suggesting that these structures are organs of great antiquity in the Vertebrate stem, and that there was formerly one present on each visceral arch. It is pointed out that were this so, it would afford a theory of the origin of the vertebrate limb, which would be supported by much of the evidence brought forward by the supporters of the Gegenbaur view, and which at the same time would avoid the most important difficulties in the way of this view.

"The Thermal Expansion of Pure Nickel and Cobalt." By A. E. TUTTON, B.Sc. Communicated by Professor TILDEN, D.Sc., F.R.S. Received April 18,—Read May 4, 1899.

(Abstract.)

The author has carried out a series of re-determinations of the coefficients of thermal expansion of these two metals with the aid of the interference dilatometer described in a former communication to the

Society.* Since the determinations made by Fizeau in the year 1869, a large amount of additional knowledge has been accumulated with reference to nickel and cobalt, including the discovery of the liquid nickel carbonyl, which places processes of purification in the hands of the chemist of a character so superior to the older methods, as to render it highly desirable that re-determinations of the physical constants of these interesting elements should be carried out with specimens of the metals thus purified By the kindness of Professor Tilden, who has prepared such specimens with infinite care for the purposes of the investigation of other physical and chemical characters, the author has been enabled to carry out determinations of the thermal expansion with rectangular blocks varying in thickness from 8· to 13 mm. The blocks were furnished with parallel and truly plane surfaces by the makers of the dilatometer, Messrs. Troughton and Simms. The range of temperature of the observations was from 6° to 121°.

The results of the determinations of the coefficients of linear expansion α are as follows —

$$\alpha = a + 2bt$$

For nickel $\alpha = 0\cdot000\ 012\ 48 + 0\cdot000\ 000\ 014\ 8t$

For cobalt $\alpha = 0\cdot000\ 012\ 08 + 0\cdot000\ 000\ 012\ 8t$.

Nine different determinations were carried out for each metal, three in each of the three rectangular directions, in order to eliminate any slight error due to directional strain in the metallic blocks. As the metals crystallise in the regular system, the expansion should be the same in all directions. The metal in each case had solidified after fusion in an oxy-hydrogen flame in presence at the last of excess of oxygen. The individual results are highly concordant, the highest result for cobalt being still lower than the lowest of the nine values obtained for nickel. Hence there can be no doubt that the above coefficients represent the true relationships

The main result of the investigation may be summarised as follows.—The coefficients of linear expansion α of pure nickel and cobalt exhibit a slight but real difference, the coefficient of nickel being distinctly greater than that of cobalt. This is true with respect to both the constant a, the coefficient for 0°, and the increment per degree, $2b$, of the general expression for the coefficient at any temperature t, $\alpha = a + 2bt$. The difference is consequently one which augments with the temperature; at 0° it amounts to 3·2 per cent., while at 120°, the upper limit of the temperatures of the observations, it attains 4·5 per cent. Similar rules apply naturally to the cubical coefficients. The metal possessing the slightly lower atomic weight, nickel, is thus found to expand to a greater extent than the metal, cobalt, which is endowed with the higher atomic weight.

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* 'Phil. Trans.,' A, vol. 191, p. 313; 'Roy. Soc. Proc.,' vol. 63, p. 208.

"On the Presence of two Vermiform Nuclei in the Fertilised Embryo-sac of *Lilium Martagon*." By ETHEL SARGANT. Communicated by Dr. D H. SCOTT, F R.S

Received April 28,—Read May 4, 1899.

In a communication to the Russian Scientific Congress, which met at Kieff, last summer, Professor S. Nawaschin summarised the brilliant results of his recent work on the fertilised embryo-sac of *Lilium Martagon* and *Fritillaria tenella* (August 30, 1898) The report of this paper, published in the 'Botanisches Centralblatt' for January 4, 1899, led Professor Léon Guignard to contribute a short account of his hitherto unpublished researches on similar stages in the life-history of some species of Lilium (*L. martagon*, *L. pyrenaicum*, and others) to the Académie des Sciences of Paris (April 4, 1899)

The results thus obtained independently by two distinguished botanists are in perfect accord, and present the greatest theoretical interest. They find that both the male generative nuclei on emerging from the pollen tube are elongated in shape, and that each is more or less twisted on its own axis. The nuclei, in fact, appear to have been killed by the fixative in the act of spontaneous movement within the embryo-sac. M. Guignard compares this motion to that of a non-ciliated antherozoid.* The "vermiform" shape can be traced in the male nucleus for some time after it has joined the nucleus of the ovum.†

The most startling discovery, however, is that the second generative nucleus unites with the upper polar nucleus of the embryo-sac, and that both then fuse with the lower polar nucleus. Thus the definitive nucleus of the embryo-sac, which later on gives rise by repeated division to the endosperm nuclei, is formed by the coalescence of three nuclei of very different origin. One is the sister-nucleus of the male element in the fertilised ovum, another, the sister-nucleus of the female element; and the third has all the characters of a vegetative nucleus. Professors Nawaschin and Guignard are in complete agreement as to these facts. M. Guignard adds that occasionally the polar nuclei have united before the arrival of the "antherozoid," and gives a number of figures in which the triple fusion is perfectly clear.

I am fortunate enough to possess a few preparations from the fertilised embryo-sac of *Lilium Martagon*, which, so far as they go, completely confirm the results of Professors Nawaschin and Guignard. The material was fixed in absolute alcohol for researches which were never even begun, but I cut a few hand sections from it immediately after fixing, to make sure that it really contained fertilised embryo-sacs. Fourteen sections were kept, all of them stained with methyl green and acid fuchsin. As this, though a brilliant, is rather a diffuse stain, I have lately re-stained eight of the preparations with Renault's hæmatoxylin and eosin, which gives more precise results.

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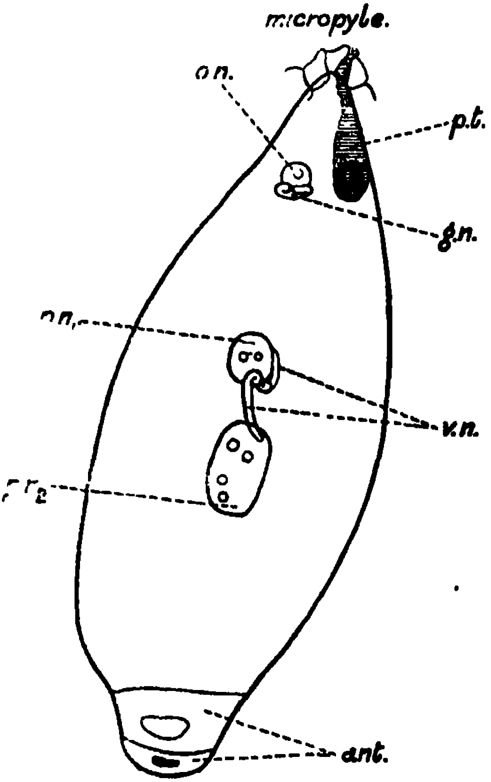
* Guignard, 'Comptes Rendus,' April 4, 1899, p. 3 of the separate copy.

† Guignard, *loc. cit.*, p. 6 and figs. 3—5, 7—11.

None of these preparations show the vermiform nuclei free in the embryo-sac. In every case conjugation has already taken place; the male nucleus is applied to the female nucleus in the micropylar end of the embryo-sac, and the second generative nucleus is applied to both polar nuclei In one case only the two polar nuclei are not in contact. The much elongated "antherozoid" unites them like a bridge, one end in contact with the lower, the other end coiled round the upper nucleus (fig. 1, *v.n*)



Excluding all doubtful cases, eight embryo-sacs show the male and female nuclei not yet fused but in contact. In six of these the male nucleus is more or less elongated. It may be distinctly coiled (fig. 1, *g.n*), or merely horse-shoe or kidney-shaped, and commonly lies on the upper or lower side of the much larger female nucleus. (See Guignard's figs. 3, 4, 8, and 10.) In two cases the male nucleus is rounded, or but slightly elongated.

Eight embryo-sacs show the polar nuclei near the centre. In five cases the mass is clearly made up of three nuclei, and the generative nucleus is distinguished from the other two by its irregular shape, the differentiation of a slender chromatic ribbon, and by the absence of a nucleolus. In three embryo-sacs two resting nuclei are applied to each other near the centre.

The pollen tube is very clear in several preparations, and it commonly contains two small nuclei, stained green, and of irregular shape. Since both generative nuclei are accounted for, these are probably due to division of the vegetative nucleus.

May 18, 1899.

The LORD LISTER, F.R.C S , D.C.L , President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them

The Bakerian Lecture, on "The Crystalline Structure of Metals," was delivered by Professor Ewing, F R S , and Mr. W. ROSENHAIN.

The following Papers were read :—

I "The Yellow Colouring Matters accompanying Chlorophyll, and their Spectroscopic Relations." By C. A SCHUNCK. Communicated by Dr. SCHUNCK, F R S

II. "The Diffusion of Ions into Gases." By J. S. TOWNSEND. Communicated by Professor J. J. THOMSON, F.R.S.

III. "The Diurnal Range of Rain at the seven Observatories in connection with the Meteorological Office, 1871—1890." By Dr. R. H. SCOTT, F.R S.

The Society adjourned over the Whitsuntide Recess to Thursday, June 1.

"On a Self-recovering Coherer and the Study of the Cohering Action of different Metals" By JAGADIS CHUNDER BOSE, M A , D.Sc , Professor of Physical Science, Presidency College, Calcutta Communicated by LORD RAYLEIGH, F.R.S. Received March 6,—Read April 27, 1899.

In working with coherers, made of iron or steel, some special difficulties are encountered in the warm and damp climate of Bengal. The surface of the metals soon gets oxidised, and this is attended with variation of sensitiveness of coherer. The sensitiveness, it is true, does not altogether disappear, but it undergoes a considerable diminution. The presence of excessive moisture in the atmosphere introduces another difficulty. Substances to be experimented on become more or less opaque by absorption of water vapour. As fairly dry weather lasts in Bengal only for a few weeks in winter, the difficulties alluded to above are for the greater part of the year serious drawbacks in carrying out delicate experiments. To avoid as far as possible the partial loss of sensibility of the receiver due to oxidation, I tried to use metals less oxidisable than iron for the construction of the coherer In my earlier experiments I derived considerable advantage by coating the steel spirals with deposits of various metals. Finding that the sensitiveness depends on the coating metal and not on the substratum, I used in my later experiments fine silver threads wound in narrow spirals. They were then coated with cobalt in an electrolytic bath. The coating of cobalt was at first apt to strip off, but with a suitable modification of the electrolyte and a proper adjustment of the current, a deposit was obtained which was very coherent. The contact surface of cobalt was found to be highly sensitive to electric radiation, and the surface is not liable to such chemical changes as are experienced in the case of steel

I next proceeded to make a systematic study of the action of different metals as regards their cohering properties. In a previous paper* I enumerated the conditions which are favourable for making the coherer sensitive to electric radiation. These are the proper adjustment of the E.M.F. and pressure of contact suitable for each particular receiver. The E.M.F. is adjusted by a potentiometer slide. For very delicate adjustments of pressure I used in some of the following experiments an U-tube filled with mercury, with a plunger in one of the limbs ; various substances were adjusted to touch barely the mercury in the other limb. A thin rod, acting as a plunger, was made to dip to a more or less extent in the mercury by a slide arrangement. In this way the mercury displaced was made to make contact with the given metal with gradually increasing pressure, this increase of pressure being capable of the finest adjustments. The circuit was completed through the metal and mercury. Sometimes the variation of pressure was produced by a pressure bulb. In the arrangement described above the contact is between different metals and mercury—metals which were even amalgamated by mercury still exhibited sensitiveness to electric radiation when the amalgamation did not proceed too far. In this way I was able to detect the cohering action of many conductors, including carbon. For studying the contact-sensitiveness of similar metals I made an iron float on which was soldered a split-tube in which the given metal could be fixed, a similar piece of metal being adjusted above the float, so that by working the plunger or the pressure bulb the two metals could be brought into contact with graduated pressure. The other arrangements adopted were the contact of spirals compressed by micrometer screw, and filings similarly compressed between two electrodes

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* "On Polarisation of Electric Ray," 'Journal of Asiatic Society of Bengal,' May, 1895.

With the arrangement described above the action of radiation on metallic contacts was studied, a brief account of which will be given under their respective groupings. It may here be mentioned that certain metals which do not usually show any contact-sensitiveness can be made to exhibit it by very careful manipulation The nature of the response of a coherer is to a certain extent modified by its condition and particular adjustment A coherer freshly made is more difficult to adjust, but at the same time far more sensitive The action is more easily under control and more consistent after a few days' rest, but the sensitiveness is not so abnormally great. The contacts of bright and clear surfaces are difficult to adjust, but such contacts are more sensitive than those made by tarnished surfaces. Pressure and E.M.F., as previously stated, also modify the reaction. For example, a freshly made and very delicately adjusted coherer subjected to slight pressure and small E M F. showed an *increase* of resistance by the action of radiation. The galvanometer spot, after a short interval, resumed its former position, exhibiting a recovery from the effect of radiation. The coherer continued to exhibit this effect for some time, then it relapsed into the more stable condition in which a diminution of resistance is produced by the action of radiation. Another coherer was found apparently irresponsive to radiation, there being the merest throb (sometimes even this was wanting) in the galvanometer spot, when a flash of radiation fell on the receiver. Thinking that this apparent immobility of the galvanometer spot may be due to response, followed by instantaneous recovery, the galvanometer needle being subjected to opposite impulses in rapid succession, I interposed a telephone in the circuit; each time a flash of radiation fell on the receiver the telephone sounded, no tapping being necessary to restore the sensitiveness. The recovery was here automatic and rapid. After twenty or thirty flashes, however, the receiver lost its power of automatic recovery, and the sensitiveness had then to be restored by tapping. An interesting observation was made to the effect that on the last occasion the receiver responded without previous tapping, a rumbling noise was heard in the telephone which lasted for a short time, evidently due to the re-arrangement of the surface molecules to a more stable condition, after which the power of self-recovery was lost

The state of sensibility described above is more or less transitory, and is induced, generally speaking, by a somewhat unstable contact and low E.M.F acting in the circuit In the majority of metals, the normal tendency is towards a diminution of contact resistance by the action of electric waves The occasional increase of resistance, in general, disappears when the pressure and E.M.F. are increased. But in the case to be presently described we have an interesting exception, where the normal state of things is just the reverse of what prevails in the majority of metals.

Alkali Metals

In the following investigations the radiator is a platinum sphere 9·7 mm. in diameter. The coherer was placed at a short distance, so that the intensity of incident radiation was fairly strong.

Potassium.—In working with this metal, the exceptional nature of the reaction became at once evident. The effect of radiation was to produce an *increase* of resistance The pressure of contact was adjusted till a current flowed through the galvanometer, the galvanometer spot of light being at one end of the scale. On subjecting the receiver to radiation the spot of light was deflected to the opposite end, exhibiting a great increase of resistance. When the pressure and E M F. were suitably adjusted a condition was soon attained, when a flash of radiation made the spot of light swing energetically in one direction, indicating an increase of resistance · the receiver, however, recovered instantaneously with the cessation of radiation, and the spot violently swung back to the opposite end, indicating the normal current that flows in the circuit This condition was found to persist, the receiver uniformly responding with an increase of resistance followed by automatic and instantaneous recovery. To prevent oxidation, the receiver was kept immersed in kerosene. When the receiver was lifted from the protecting bath, it still continued to respond with an increase of resistance, but with a gradual loss of power of automatic recovery. This power was again restored on again immersing the coherer in kerosene. The receiver in vacuo, or under reduced hydrogen pressure, would have been preferred, had the necessary appliances been available.

Sodium.—As we pass from potassium to the neighbouring metals, there is a gradual transition of property as regards the nature of response to electric waves. With sodium the adjustment is a little more difficult than with potassium, but the response is somewhat similar to that of potassium. Though in general there is an increase of resistance produced by electric radiation, there are occasional exceptions when a diminution of resistance is produced. With some trouble the adjustment could be made so that the recovery is also automatic, but it is not so energetic as in the case of potassium

Lithium.—Specimens of this metal not being available, I obtained a deposit of it on iron electrodes by electrolysis of the fused chloride. The action produced by electric radiation was sometimes an increase and sometimes a diminution of resistance, the increase of resistance being the more frequent. With some difficulty it was possible to adjust the sensitiveness so that the recovery was automatic, but it was not energetic nor did this power persist for a long time.

Metals of the Alkaline Earth

Pure metals of this group being not available, I had to rely on the deposit obtained by electrolysis Chloride of calcium was fused in a crucible, and deposits were produced on iron cathodes, the anode being a carbon rod. The deposit was not very even. One of the iron rods with the deposit was tested by immersion under water, when hydrogen was evolved I did not succeed in getting deposits of either barium or strontium, the temperature available not being sufficiently high.

On making a coherer with calcium, and keeping it immersed in kerosene, an action similar to that produced by sodium was observed. The tendency of self-recovery was, however, very slight.

Magnesium, Zinc, and Cadmium.

In these metals and in the succeeding groups there is a pronounced tendency towards a diminution of resistance by the action of electric radiation. Magnesium being easily oxidisable, there is a thin coating of oxide on the surface. When this is scraped, the metal makes a very highly sensitive receiver. The adjustment is not difficult, the metal allowing a considerable latitude of pressure and E.M.F. It has already been stated that the metals which are slightly tarnished can be more easily adjusted.

Though there is in this metal a decided tendency towards a reduction of contact resistance, yet it is possible by careful adjustment to obtain an increase of resistance. Indeed it is sometimes possible to so adjust matters that one flash of radiation produces a diminution of resistance, and the very next flash an increase of resistance. Thus a series of flashes may be made to produce alternate throws of the galvanometer needle. The more stable adjustment, however, gives a diminution of resistance, and receivers made with this metal could be made extremely sensitive. The tendency towards recovery is almost wanting.

Zinc.—This metal also exhibits moderate sensitiveness; it, however, requires a more careful adjustment.

Cadmium.—The action of this metal is somewhat similar to that of zinc, but the sensitiveness is very much less.

Bismuth and Antimony.

Both bismuth and antimony make very sensitive receivers. Moderately small E M.F. with slight pressure is best suited for these metals.

Iron and the Allied Metals

Iron.—The action of this metal is well known. In one of my experiments I used it in connection with mercury. When the contact is very lightly made, there is a tendency towards an increase of resistance by the action of radiation. But after a time the action became normal, that is to say, there was a diminution of resistance.

Nickel and Cobalt.—These are also very sensitive. The surface being bright, the E.M.F. and pressure are to be adjusted with some care.

Manganese and Chromium—These were obtained in the form of powder. Their action is similar to the other metals of this group

Aluminium.—This also makes a sensitive receiver.

Tin, Lead, and Thallium.

It is somewhat difficult to adjust *tin*, but when this is done the metal exhibits fair sensitiveness. *Lead* is also sensitive. The sensitiveness of *thallium* is only moderate

Molybdenum and Uranium.

The specimen obtained was in the form of powder, and very tarnished in appearance. The sensitiveness exhibited was slight..

Metals of the Platinum Group.

Platinum exhibited a moderate amount of sensitiveness. Spongy platinum also showed the same action. The absorption of hydrogen made the action slightly better, but the improvement was not very marked.

Palladium.—This made a more sensitive coherer than platinum. The adjustment is, however, more troublesome.

Osmium.—The specimen was in the form of powder. It requires a higher E.M.F. to bring it to a sensitive condition. The sensitiveness was moderate.

Rhodium was found to be more sensitive than osmium.

Copper, Gold, and Silver.

Copper required a much smaller E.M.F. The sensitiveness was only moderate.

Gold was more difficult to adjust, but the action is a little stronger.

Silver.—The receiver was extremely unstable. It exhibited sometimes a diminution and at other times an increase of resistance.

It will be seen from the above that all metals exhibit contact sensitiveness to electric radiation, the general tendency being towards a diminution of resistance.

The most interesting and typically exceptional case, however, is the receiver made with potassium, which not only exhibits an increase of resistance by the action of radiation, but also a remarkable power of self-recovery In the accidental instances of increase of resistance exhibited by other metals, an increase of pressure or E.M.F. generally brought the coherer to the normal condition, which showed a diminution of contact resistance by the action of electric waves. With potassium I gradually increased the pressure till the receiver grew insensitive. All along it indicated an increase of resistance, even when one piece was partially flattened against the other I increased the E.M.F. many times the normal value, this increase (till the limit of sensitiveness was reached) rather augmented the sensibility and power of automatic recovery. I allowed the receiver a period of rest, the nature of response remaining the same. As far as I have tried, potassium receivers always gave an increase of resistance, a property which seems to be characteristic of this metal, and to a less extent, of the allied metals.

It will thus be seen that the action of potassium receiver is not, strictly speaking, a cohering one. For it is difficult to see how a cohering action and consequent better contact could produce an increase of resistance. It may be thought that the sudden increase of current may, by something like a Trevelyan rocker action, produce an interruption of contact. But such a supposition does not explain the instantaneous action, and the equally instantaneous recovery.

In arranging the metals according to their property of change of contact resistance, I was struck by the similarity of action of electric radiation on potassium in increasing the contact resistance, and the checking action of visible radiation on the spark discharge. In the latter case too potasium is also photo-electrically the most sensitive. But the action is confined to visible radiation, and is most efficient in the ultra-violet region. I was indeed apprehensive that the action on potassium receiver which I observed might be in some way due to the ultra-violet radiation of the oscillatory spark. But this misgiving was put to rest from the consideration that the receiver was placed in a glass vessel filled with kerosene, through which no ultra-violet light could have been transmitted. To put the matter to final test, I lighted a magnesium wire in close proximity to the receiver without producing any effect Thick blocks of wood of ebonite and of pitch were interposed without checking the action I then used polarised electric radiation, and interposed a book analyzer, 6 cm. in thickness; when the analyzer was held parallel, there was a vigorous action, but when it was held in a crossed position all action was stopped. No visible or heat radiation could have been transmitted through such a structure, and there can be no doubt that the action was entirely due to electric radiation.

It would be interesting to investigate whether the observed action of electric radiation on a potassium receiver is in any way analogous to the photo-electric action of visible light. I have commenced an investigation on this subject, the results of which I hope to communicate on another occasion.

BAKERIAN LECTURE.—"The Crystalline Structure of Metals." By J. A. EWING, F.R.S., Professor of Mechanism and Applied Mechanics in the University of Cambridge, and W. ROSENHAIN, 1851 Exhibition Research Scholar, Melbourne University. Delivered May 18, 1899

(Abstract.)

In a previous communication, read to the Society on March 16, a preliminary account was given of some of the results the authors had arrived at in studying metals by the microscopic methods initiated by Sorby, and pursued by Andrews, Arnold, Behrens, Charpy, Osmond, Roberts-Austen, Stead, and others The present paper deals with a development and extension of the same work. It relates chiefly, though not exclusively, to the effects of strain, and the relation of plasticity to crystalline structure.

It is well known that the etching of a polished surface of metal reveals, in general, a structure consisting of irregularly shaped grains, with clearly marked boundaries. Each grain is a crystal, the growth of which has been arrested by its meeting with neighbouring grains. This view, as Mr. Stead has pointed out, is strongly supported by the appearance of the etched surface under oblique illumination, when the several grains are seen to reflect light in a way which is consistent only with the idea that on each there is a multitude of facets with a definite orientation, constant over any one grain, but different from grain to grain. The formation of such a structure is well exhibited, on a relatively enormous scale on the inner surface of a cake of solidifying

bismuth, from which the still molten metal has been poured away. Another striking example of this structure is seen in steel containing about 4½ per cent. of silicon. The fractured ingot of this material exhibits large crystals, and by deeply etching a polished surface Mr. Stead has obtained a beautiful development of the regularly oriented elements of which the crystalline grains are built up* on a scale so large as to require but little magnification

The authors have obtained much evidence that this structure is typical of metals generally Probably under no condition does any metal cease to be crystalline.

The crystalline character of wrought-iron bars or plates is seen when the polished surface is etched, not merely by the general appearance of the grains under oblique light, but by the development of geometrical pits on the surface. These pits have a definite orientation over each grain, and the orientation changes from one grain to another. Usually in the purest commercial iron their outline is that of plane sections of a cube, but occasionally they are apparently plane sections of an octahedron. In some instances isolated and comparatively large pits only are seen, in others nearly the whole surface of a grain, when viewed under a magnification of 1000 or 2000 diameters, is found to be covered with small as well as large pits, geometrically similar and similarly oriented. Photographs of these are given in the paper.

For the purpose of producing smooth surfaces in the more fusible metals, without polishing, the metal was poured in a molten state on a plate of smooth glass. The surface produced in this way shows well the boundaries between the grains, and in some cases it also exhibits the crystalline character of the grains in a remarkable way by means of geometrical pits, which are apparently formed on the surface in consequence of the presence of small bubbles of air or, more probably, of gas given out from the metal itself during solidification. Cadmium shows these particularly well, and they are to be observed also in tin and zinc. These air-pits are seen, under 1000 diameters, to be negative crystals, similar and similarly oriented on each grain, and, in cadmium, to have outlines which suggest that they are sections of hexagonal prisms. Their characteristics are exhibited in the photographs, which also show how the boundaries between the grains are emphasised by the collection there of air or of gas given off by the metal during solidification. The true boundary is merely the trace of a surface on a plane, but it may be broadened out in this way into a wide shallow channel.

The effects of strain have been examined in many metals, using surfaces prepared either by polishing or by casting against a smooth plate. When any metal is strained beyond its elastic limit in any way, the surface of each crystalline grain becomes marked by one or more systems of lines running in a generally straight and parallel fashion over it. The direction of the parallel lines changes from grain to grain. Thus those lines serve to mark out one grain from another in a metal which, although polished before straining, has not been etched to develop the boundaries. As straining proceeds, the lines become more and more numerous and emphatic, and two, three, or four systems appear on each grain.

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* 'Journal of the Iron and Steel Institute,' 1898.

The nature of those lines has been described in the authors' paper of March 16 They are slips along cleavage or gliding planes in the crystals. The effect of each slip is to develop a step on the polished face. The short inclined surface forming this step looks black under vertical illumination, but shines out brightly when oblique light of a suitable incidence is used These slip bands, as they were named in the previous paper, are thus seen as narrow dark or bright bands, accordingly to the nature of the lighting.

The authors have developed slip-bands in iron, copper, gold, silver, platinum, lead, tin, bismuth, cadmium, aluminium, nickel, as well as steel, brass, gun-metal, and various other alloys. So far as the observations go, they occur in all metals.

The slip-bands are in themselves an evidence of crystalline structure, and, further, they show how such a structure is consistent with plasticity, and how it persists after plastic strain has occurred. The "flow" or non-elastic strain of a metal occurs through numerous finite slips taking place on the cleavage or gliding surfaces in each of the crystalline grains of which the metal is an aggregate. The elementary pieces which slip on one another retain their primitive crystalline character.

Further, if the movement of the pieces with respect to each other in any one grain is a movement of translation only, their orientation should remain uniform in each grain.

That this is actually the case is demonstrated by examining specimens of metal which had been violently deformed without any subsequent annealing or heating. In metal that has been rolled or hammered in the cold state, or deformed by tension or compression or strain of any kind, however severely, the grains are still seen where a surface is polished and etched. Their form is much changed by the strain which the piece has undergone. But the fact that they have retained their crystalline structure is demonstrated when, after polishing, the piece is subjected to a slight additional strain of any kind, for the effect of this additional strain is to develop slips of the same general character as before. Further evidence to the same effect is given by the fact that etching the polished surface of a very severely strained piece develops geometrical pits, which are similar and similarly oriented over the face of each grain, notwithstanding the great distortion which the grain has suffered as a whole. The effects of oblique lighting in metal which is polished and etched after severe straining are referred to as illustrating the same point. The persistence of crystalline structure is demonstrated by micro-photographs of the section of a bar of Swedish iron which had been rolled cold from a diameter of ¾ inch to a diameter of ½ inch without subsequent heating. The outline of the grains is much distorted, but the orientation of the crystalline elements remains constant within each individual grain.

The slips in metals which exhibit a cubical crystalline structure on etching are in some instances parallel to the faces of the cubes, and are very frequently inclined to the faces, apparently along the octahedral planes. Stepped lines are frequently seen, and also lines which appear curved probably in consequence of numerous steps which are unresolved even under the highest powers In exceedingly plastic metals such as lead, copper, and gold, the lines are particularly straight. A piece of lead cast against glass to produce a smooth surface gives, when slightly strained, a splendid display of slip-bands, and the boundaries of the grains are sharply defined by the meeting of the lines on one grain with those on its neighbours Another way to get a clear lead surface for the purpose of showing slip-bands is to press a freshly cut piece of the metal with considerable force against a smooth object. Photographs of slip-bands in iron, gold, silver, lead, copper, and other metals are given in the paper.

When a metal is fractured the grains do not as a rule part company at their boundaries, but split along cleavage surfaces. It is to this that the crystalline appearance, obvious in many fractures, is due.

In several metals the authors find that "twinning" takes place in the crystalline structure as an effect of strain. Samples of copper, which in the original cast state gave no evidence of the existence of twin crystals, were hammered or otherwise wrought, and were then found to be full of twins The twinning produced in this way survived after the wrought copper had been raised to a red heat and allowed to cool. Similar results were obtained in gold and in silver; the metal in the cast state did not show twins, but they were found after the metal had been wrought and subsequently softened by annealing. An example of twinning was observed in nickel after the application of a somewhat severe strain. Twins were readily developed in cadmium by strain, apparently as a result of the slight strain which was applied for the purpose of developing slip bands They were also found in lead, zinc, and tin, either as a primitive feature in the crystallisation or produced by straining. The twinning frequently takes the form of a large number of parallel bands within a single grain, and a twin band due to strain in one grain is sometimes associated with a twin band in neighbouring grains, the bands being continuous except for a change in orientation in passing from grain to grain.

Photographs of twin bands in copper, gold, lead, and other metals are given, showing the twin bands as revealed by a cross-hatching of parallel slip lines, the sets of lines being parallel to one another in alternate bands of the twin. The twinning under strain which we have observed in various metals is similar to that which is known to occur in calcite. It may be regarded as a result of slip accompanied by a definite and constant amount of rotation on the part of the molecules

From this point of view there are two modes in which plastic yielding occurs in a crystalline aggregate One is by simple slips, where the movements of the crystalline elements are purely translatory and their orientation is consequently preserved unchanged. The other is by twinning, where rotation occurs through an angle which is the same for each molecule in the twinned group. Both modes are often found not only in a single specimen of metal but in the same crystalline grain.

At the suggestion of Messrs Heycock and Neville, the authors' examination of the effects of strain has been extended to certain eutectic alloys. The structure of such alloys has already been described by Osmond, with whose observations these are in agreement. The alloy generally exhibits rather large grains, the structure of which is very different from that of pure metals, for it consists of an intimate intermixture of two constituents, one of which appears as separate or dendritic crystals on a field formed of the other constituent. The two are seen forming an exceedingly minute and complex structure within each of the large grains of which the alloy is made up Straining has the effect of making this intimate structure more apparent, by causing slips which set up differences of level between pieces of one and the other constituent

A study of the micro-structure of alloys suggests a possible explanation of the peculiarities they present in regard to variation of electrical conductivity with temperature. The two constituents may behave individually as pure metals in this respect, but if their coefficients of expansion are different the closeness of the joints between them will depend on the temperature. Thus if the more expansible metal exists as plates, or separate pieces of any form within the other, the effect of heating will be to make the joints between the two conduct more readily, with the result of reducing the increase of resistance to which heating would otherwise give rise, and in extreme cases with the effect even of producing a negative temperature coefficient. The high resistance of alloys generally may be ascribed to the large number of joints across which the current has to pass

In casting metals against glass and other smooth bodies for the purpose of getting a surface fit for microscopical examination, a surface is occasionally produced which not only shows the true boundaries between the crystalline grains, but also additional markings which simulate boundaries in a very curious manner. These pseudo-boundaries are often polygonal in form, like the real boundaries, and have an intimate geometrical association with them. Under low powers they are in some instances difficult to distinguish from true boundaries; but the distinction is apparent under high powers, and it becomes obvious as soon as slip-bands are developed by the straining of the metal The pseudo-boundaries are found to consist in small variations of level in the surface of the grains in which they occur. Their form suggests that they are projections upon the surface of real edges below. They occur very conspicuously in cadmium, especially when it is cast on a cold surface, and less conspicuously in zinc It is probable that in the strain set up by unequal cooling after the metal has solidified, the lower edges of the crystalline grains project a sort of image of themselves on the surface by slips, or possibly by narrow bands of twinning. The effect resembles that of a Japanese "magic" mirror, in which slight inequalities of the surface, corresponding to a pattern behind, cause light reflected from the mirror to produce an image in which a ghost of the pattern may be traced.

The authors regard their experiments as establishing the conclusion briefly stated in their previous paper, to the effect that the plasticity of metals is due to the sliding over one another of the crystalline elements composing each grain, without change in their orientation within each grain, except in so far as such change may occur through twinning.

"The Yellow Colouring Matters accompanying Chlorophyll, and their Spectroscopic Relations" By C A SCHUNCK Communicated by EDWARD SCHUNCK, F R S. Received April 20, —Read May 18, 1899.

[PLATE 6]

The yellow colouring matters dealt with are those accompanying chlorophyll in healthy green leaves and which are extracted along with it by means of boiling alcohol.

This group of yellow colouring matters is generally known by the name xanthophyll, a term first used by Berzelius, who was the first observer to express the belief that a yellow colouring matter pre-exists along with the green colouring matter in alcoholic extracts of green leaves. The subject has subsequently received the attention of many investigators—Fremy, Michels, Millardet, Muller, Tinisnaseff, Gerland, Raunenhoff, Askenasy, Stokes, Sorby, Tschirch, Kraus, Filhol, Hansen, and Schunck. The principal results arrived at by these investigators are as follows:—Filhol noticed that by treating crude alcoholic chlorophyll solutions with animal charcoal it is possible to remove the green constituent of the mixture when a yellow coloured solution remains, the colour of which he believes is evidently due to a pre-existing colouring matter or matters associated with the green one. Kraus—to whom we are indebted for a most elaborate study of the physical properties of the yellow constituent of crude chlorophyll solutions—confirmed the observations of Filhol, and added a number of new ones which lead, according to him, to an explanation of the absorption spectrum of crude chlorophyll solutions which has hitherto been universally accepted as the correct one. The author used amongst other methods, for the purpose of separating the yellow colouring matters from the green, their different solubility in alcohol and benzol or, correctly speaking, benzoline. An alcoholic solution of chlorophyll, treated with benzoline, retains, according to him, the yellow colouring matter or mixture of colouring matters, while the benzoline takes up the green constituent. By an investigation of the spectroscopic properties of these solutions, compared to the original one, Kraus arrived at the result that the ordinary chlorophyll spectrum, which has been described with considerable accuracy already by Brewster, is a complex one, *i e*, that some of the absorption bands are due to the green constituent and some to the yellow. The former, he says, *is characterised by six bands, four of which (comprising the well*-known chlorophyll spectrum) are situated between the solar lines B and E; the fifth between F and G, and the sixth in front of G. The yellow constituent shows two bands, one at F or just behind it, and the second in front of G. These observations, according to Kraus, explain the constitution of the spectrum of crude chlorophyll solutions, the first four bands of which being due solely to the green constituent, the fifth to the yellow, and the sixth to a combination of the sixth band of the green constituent and the second of the yellow. The fifth band of the green constituent being very faint, and situated between the fifth and sixth bands of the mixture, does not, according to him, appear at all. These explanations, however, as will be shown, are erroneous.

Sorby using carbon bisulphide as the separator in place of benzoline states that along with chlorophyll in the crude alcoholic extracts of the green leaves of the higher plants there are three accompanying yellow colouring matters present which he names orange xanthophyll, xanthophyll, and yellow xanthophyll, each showing a couple of bands in slightly different positions in the more refrangible visible portion of the spectrum, but none in the less refrangible part, and also that there are other yellow colouring matters present, which he groups under the term lichnoxanthine, which obscure the more refrangible portion but exhibit no bands. He also states that chlorophyll (the green constituent) of the higher plants is separable by the same means into two colouring matters which he terms "Blue Chlorophyll" and

"Yellow Chlorophyll," the former being the chief constituent, the latter being present in only a small relative quantity, and each give a series of bands situated throughout the visible portion of the spectrum.

Hansen's method of isolating the yellow colouring matters is different from those of the previous observers. He treats the alcoholic extracts of green leaves with caustic alkali, evaporates the liquor to dryness, and extracts from the residue the yellow colouring matter by means of ether, the study of which lead him to believe that the yellow constituent shows only two bands.

Schunck obtains from all crude alcoholic chlorophyll extracts minute sparkling red crystals which are deposited on standing, and to which he has not applied a name, but which he considers identical with the erythrophyll of Bougarel and the chrysophyll of Hartsen. On dilution the yellow solutions of these crystals gave two absorption bands in the more refrangible portion of the spectrum, but none in the less refrangible, and, though not in the same positions as the similar bands (the fifth and sixth) shown by crude chlorophyll solutions, he considers these latter bands not due to chlorophyll but to an accompanying yellow colouring matter.

Finally, Tschirch, who used Hansen's method for separating the yellow from the green constituent, describes two yellow colouring matters, to which he gives the names xantho-carotin, showing three bands in the more refrangible part of the spectrum and to which, according to him, the bands in the blue and violet shown by crude chlorophyll solutions are due, and xanthophyll proper, which shows no bands whatever but only a total obscuration in the violet region

It will be seen that the results obtained by the various observers do not agree, and a renewed study of the yellow constituent of crude chlorophyll solutions appeared to be desirable. My own results differ in many respects from the hitherto generally accepted ones, they relate not only to the physical nature of the yellow colouring matters in question, but also enable us to characterise chlorophyll proper in a different manner than was possible before. The preparation of pure chlorophyll seems to baffle all attempts, but so far the physical properties of this substance, the knowledge of which would guide an experimenter in reaching the goal have, as it proves, not been known with sufficient completeness.

I will now give the results of the experiments I have made, in the endeavour to separate these yellow colouring matters from the accompanying chlorophyll, dealing more especially with their spectroscopic relations as compared to those of chlorophyll in the violet and ultraviolet region of the spectrum investigated by aid of photography—a means which, with the exception of Tschirch, former observers have not applied—and by which means I have been able to ascertain one or two new facts, and have, I think, been able to clear up the much debated point whether the absorption bands in the violet and ultra-violet region shown by crude chlorophyll solutions are due to chlorophyll itself or to the accompanying yellow colouring matters.

The chlorophyll solutions experimented upon were obtained in the usual manner by extracting the colouring matter from the leaves with boiling alcohol. I have already shown* that chlorophyll solutions prepared in this way show three characteristic absorption bands on proper dilution, in the violet region of the spectrum, giving in the less refrangible region the well-known spectrum of four bands which in very pure solutions may be said to be reduced to three, so faint does the fourth band appear

If the extracts are concentrated enough one finds invariably on standing for a day or so minute sparkling red crystals deposited on the sides of the containing vessel or along with the fatty deposit, coloured green by chlorophyll, which generally comes out of the extracts on standing These crystals are found in variable quantities, but more often than not in a minute quantity

This is the first yellow colouring matter one comes across and is the erythrophyll of Bougarel, and the chrysophyll of Hartsen and Schunck.† That chrysophyll is always to be found in chlorophyll solutions proves that either it pre-exists as such along with chlorophyll in its alcoholic extracts, or that it is formed spontaneously from one of the colouring matters, and is not, according to Hansen,‡ formed under certain conditions *only* by the decomposition of a derivative. Chrysophyll thus obtained is not a very stable substance, and in order to preserve it unchanged it should be placed in a glass tube through which a current of hydrogen has been passed before sealing, and kept in the dark Its alcoholic solutions are bleached rapidly when exposed to the air and sunlight, and even when kept in the dark a change very soon takes place in its solutions as shown by its spectrum, though there is no apparent change in colour. According to Arnaud§ it is identical with carotin. Chrysophyll gives no absorption bands in the red, yellow, or green, but three very distinctive bands in the violet region of the spectrum which, as I have shown,‖ are almost identical in position with those of carotin They (Plate 6, fig 5) occupy intermediate positions compared to the three bands shown by crude chlorophyll solutions in the same region, being shifted more towards the red end of the spectrum.

The method I have applied for separating the other accompanying yellow colouring matters from the chlorophyll is that of treating the crude alcoholic extracts with an excess of animal charcoal in the cold for about an hour, which removes all the chlorophyll and leaves a yellow solution, which gives no absorption bands in the red, yellow, or green, but four distinctive bands situated in the violet and ultra-violet region of the spectrum. The prolonged action of animal charcoal has the effect of ultimately absorbing all the colouring matters, leaving the solution colourless By this means the yellow colouring matters can be obtained free from chlorophyll, but I have not been able to recover the latter from the animal charcoal This was tried by boiling the charcoal with ether, the result being a greenish yellow solution, giving the four bands in the violet as before, but, in addition, a faint band in the red, showing that it consisted of a portion of the yellow colouring matters which had been absorbed by the charcoal together with a trace of chlorophyll which caused the greenish colour, and the faint band in the red region of the spectrum.

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* 'Roy. Soc. Proc.,' vol. 63, p 393.

† 'Roy Soc Proc.,' vol 44, p. 440

‡ 'Die Farbstoffe des Chlorophylls,' 1889, p. 58.

§ 'Compt. Rend.,' vol. 102, p. 1119, and vol. 104, p. 1293.

‖ 'Roy. Soc. Proc.,' vol. 63, p. 393.

Yellow solutions obtained in this way from some chlorophyll extracts—which, when freshly prepared, show signs of decomposition, viz., the fourth band in the visible region of the spectrum darker and the third fainter—show only the first two or three bands in the violet region, the rest of the violet and ultra-violet being obscured. In such cases a separation can be effected by ether, the yellow colouring matter causing the obscuration remaining in the alcoholic portion, the yellow ethereal portion now showing the four-banded spectrum as before This colouring matter causing obscuration no doubt belongs to the lichnoxanthine group of Sorby,* and corresponds to the so-called xanthophyll of Tschirch † These yellow solutions deposit on spontaneous evaporation an amorphous substance impregnated with much fatty matter, which so far I have failed to remove and have been unable to get in a crystalline form. It is insoluble in water, but easily soluble in alcohol and ether, giving as before the distinctive spectrum of four absorption bands in the violet and ultra-violet, but *no* bands in the red, yellow, or green; these bands, with the exception of the first, which is almost if not quite identical in position with the first band shown by crude chlorophyll solutions in the violet region, are in distinctly different positions to the two remaining chlorophyll bands, or the three due to chrysophyll (figs. 1 to 5). It appears to be much more stable than chlorophyll, resisting the action of light and air to a greater extent; even from crude chlorophyll solutions which have been kept for some time and show distinctly from their spectra the formation of phyllocyanin, it can be obtained unaltered by the action of animal charcoal. The solutions, when exposed to sunlight, gradually become colourless, but at a less rapid rate than those of chrysophyll, and apparently without the formation of products of decomposition, as is the case with chlorophyll; while away from the light it can be kept for a considerable time without any apparent change taking place in its solutions. Alkalis which induce so great a change in chlorophyll appear to have no action upon it. From chlorophyll solutions which have been boiled with potash or soda, and from which animal charcoal will not now absorb any appreciable amount of colouring matter, ether takes up this yellow colouring matter unaltered. Likewise, if its alcoholic solutions be boiled with an alkali, no alteration is discernible. On the other hand, if hydrochloric acid gas be passed through its alcoholic solutions the colour changes to a dull dark-red, giving no bands, but a general obscuration in the violet and ultra-violet region of the spectrum. By this method I have obtained this yellow colouring matter from two species of *Ficus*, from parsnip, clover, birch, and Virginia creeper leaves, the extracts of the last-named, when even freshly prepared, showing a near approach to the phyllocyanin spectrum. I have also examined the yellow colouring matter of autumnal leaves, and find that it is identical with it both in properties and spectrum (Plate 6, figs 3 and 4); but in autumnal leaves invariably I find the presence of the other yellow colouring matter which obscures the spectrum, but which can be got rid of by separating with ether. This supports the belief that the yellow colouring matter of autumnal leaves is what remains after the chlorophyll of the healthy green leaf has faded away, the latter being the less stable of the two (as has already been shown to be the case), and fading first. I have examined the colouring matter of some etiolated leaves, and here the presence of a yellow colouring matter that obscures the spectrum in the violet and ultra-violet region is undoubted. I endeavoured to get rid of it, as in the former experiments, by separating with ether, but seemingly with only partial success, the ethereal portion showing only three bands, but in the same positions as the first three bands of the yellow colouring matter under review, the rest of the spectrum in the violet and ultra-violet being obscured.

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* 'Roy. Soc. Proc.,' vol. 21, p. 462.

† 'Ber. der Deutsch. Bot. Ges.,' vol. 14, part 2. p. 76. 1896.

It is to this yellow colouring matter giving the characteristic spectrum of four bands in the violet region, a spectrum not before observed, I believe, and which I believe to be the predominating yellow colouring matter accompanying chlorophyll in ordinary green leaves, and also of faded autumnal leaves, that I would restrict the name xanthophyll, just in the same way that phylloxanthin, from being first applied by Fremy to include *all* the yellow colouring matters accompanying chlorophyll, as well as a yellowish-brown decomposition product of the latter, has been applied by Schunck in a stricter sense to one of the group only—the yellowish-brown decomposition product of the action of acids. Crude alcoholic chlorophyll solutions can, as before stated, be separated into a green and yellow portion by agitating with carbon bisulphide—Sorby's method—or by benzoline, Kraus's method, the carbon bisulphide and benzoline portions being coloured green, and the alcoholic portion in each case yellow. I believe from the few experiments I have so far made by these methods of separation, that the xanthophyll, as I have defined it, passes along with the chlorophyll into the carbon bisulphide or the benzoline, a point overlooked by Kraus, but noticed by Sorby, while the alcoholic portion contains yet another yellow colouring matter or matters, showing ill-defined absorption bands in the violet region, but in different positions again to either the bands of chrysophyll or xanthophyll. May be we have here the xanthophyll and yellow xanthophyll of Sorby,* whilst probably my xanthophyll corresponds to his orange xanthophyll.

I hope after some further experiments by these means of separation which I am now undertaking, to be able to throw some further light upon the apparent complex nature of this group of accompanying yellow colouring matters.

The Spectroscopic Relations of Chlorophyll, Chrysophyll, and Xanthophyll.

The method of observing the absorption spectra by means of photography is the same as I adopted in a former investigation, dealing with chlorophyll and its derivatives,† quartz lenses and an Iceland spar prism being used. The question whether the bands shown by crude chlorophyll solutions in the violet region are due to chlorophyll itself, or to an accompanying yellow colouring matter, has not so far been answered in a satisfactory manner. Some observers consider them due to the former, whilst others, the majority I believe, attribute them to the latter.

On inspection of the plate (figs 1, 2, and 5), it will be seen that the chrysophyll bands are shifted towards the red end of the spectrum compared to those of crude chlorophyll, so that if chrysophyll preexists along with chlorophyll in its alcoholic extracts, the bands of the two spectra would overlap, so as to produce one broad band extending from F to K_β; but in all the freshly prepared normal crude chlorophyll extracts I have examined, I have never found this to be the case, the three bands always being visible on proper dilution, with no indication of those due to chrysophyll.

We can therefore conclude that they are not due to chrysophyll, and must assume that if chrysophyll pre-exists, its relative quantity compared to chlorophyll must be small. In the xanthophyll spectrum, it will be noticed (Plate 6, fig. 3) the first three bands do not coincide with the chrysophyll bands, being shifted towards the ultra-violet, and the spectrum is further distinguished by a fourth band situated between K_β and L, which is lacking in chrysophyll, the latter having the characteristic of allowing considerably more of the ultra-violet rays to pass through its solutions. Comparing now the xanthophyll spectrum with that given by crude chlorophyll solutions, it will at once be seen that the character of the two spectra are quite different, and this points to the fact that the bands are due to two distinct colouring matters. This is supported by the fact that from crude alcoholic chlorophyll solutions that have been acted upon with alkali, the xanthophyll can be recovered unaltered by shaking up with ether, the chlorophyll being so altered (though the colour of the solutions is unchanged) that animal charcoal will not now absorb the green colouring matter, this alteration has been shown by Schunck* to be due to the formation of an alkali compound of alkachlorophyll. Again, in crude chlorophyll solutions which have been kept a little time, and which show signs of decomposition, owing to the formation of phylloxanthin and phyllocyanin, the bands in the violet are no longer discernible on proper dilution, yet act upon such solutions with animal charcoal, and we get a yellow solution giving the four-banded xanthophyll spectrum, showing that the xanthophyll was there all the time unaltered, but did not affect the spectrum From the spectra of their alcoholic solutions it will be observed the only xanthophyll band that could affect the crude chlorophyll spectrum is the first, for it is the only one in a similar position, but I think I can show that this cannot be the case, and that it is in no way connected with chlorophyll If a crude chlorophyll solution be examined in ether compared to alcohol, it will be found that in the former solvent, the bands are all shifted towards the more refrangible end of the spectrum, while the xanthophyll bands in both solvents are in identical positions (Plate 6, figs. 6 to 11), so that we see now the first xanthophyll band no longer coincides with the first band of crude chlorophyll, and therefore can not have any influence upon the chlorophyll spectrum. And thus I came to the conclusion, taking one experiment with another, that as with chrysophyll, xanthophyll is present in such a small relative quantity, compared to chlorophyll, that the bands of its spectrum cannot be detected in the crude chlorophyll solution, and that the bands shown by the latter are due to chlorophyll itself, and not to any of the accompanying yellow colouring matters which the majority of former observers believed they were due to. This belief may have arisen from the fact that without the aid of photography, only the first two bands of chlorophyll and the accompanying yellow colouring matters in the violet region are discernible in alcoholic solutions to the eye, and they thus failed to observe the complete spectrum which, from an inspection of the plate, will be seen makes in each case a vast difference to the character of the spectrum.

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* 'Roy. Soc Proc,' vol. 21, p. 466.

† 'Roy. Soc Proc.,' vol. 63, p. 391.

That all freshly prepared chlorophyll extracts show in every case

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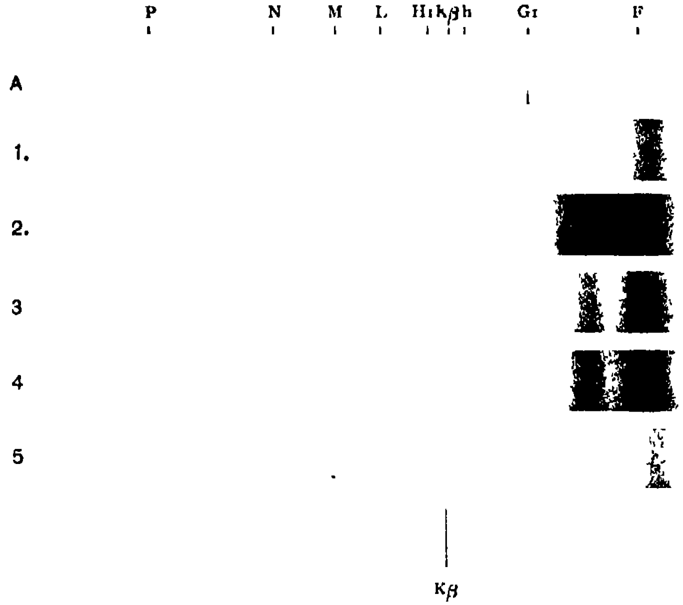
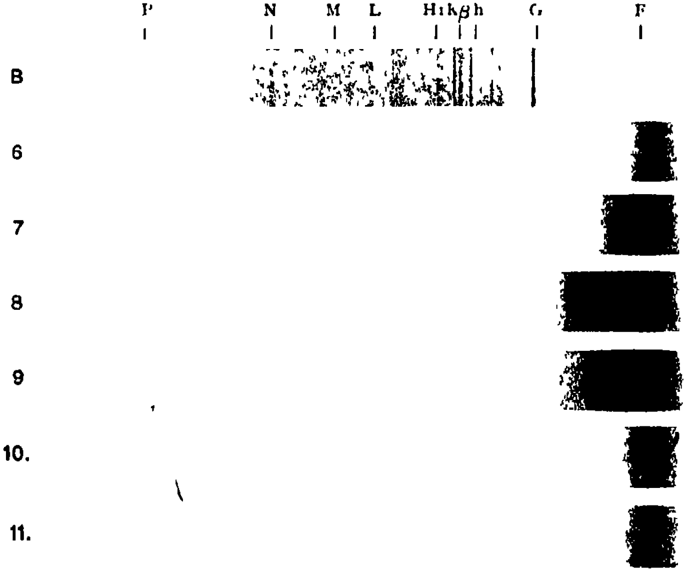
* 'Roy. Soc. Proc,' vol. 50, p. 312.


P
N
M
L
H₁ k β h
G₁
F
A
1.
2.
3
4
5
Kβ


P
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10.
11.

the three bands in the violet region, is I think a proof in itself that they are due to the same colouring matter that produces the characteristic and unmistakable spectrum in the less refrangible region, or to a *very* intimately connected colouring matter so far unknown. Seeing also that all the chlorophyll derivatives give a characteristic absorption in the violet and ultra-violet region, it would be strange that chlorophyll proper should prove the exception, and it is an interesting fact that its two chief, and intimately connected decomposition products *inter se*—phyllocyanin and phylloxanthin—should have, as I have shown,* bands in the violet region in identical positions with these three chlorophyll bands, the first of them corresponding to a faint, but distinct one, observable in pure phyllocyanin solutions on proper dilution, the other two to the two characteristic phylloxanthin bands in the violet region

Summary

1. I find in *all* crude alcoholic extracts of healthy green leaves, two yellow colouring matters accompanying the chlorophyll One chrysophyll which deposits out of the extracts on standing in lustrous red crystals, but more often than not in minute quantities, the other obtained by treating the extracts with animal charcoal in the cold, the charcoal taking up the chlorophyll, and leaving a yellow solution which deposits on spontaneous evaporation an amorphous substance impregnated with much fatty matter, and to which I have restricted the name xanthophyll. Another yellow colouring matter is sometimes found along with xanthophyll which gives no absorption bands, but only an obscuration in the violet and ultra-violet region of the spectrum, and in such cases a separation can be effected by ether. There is also evidence to show that other yellow colouring matters may exist. Xanthophyll, however, I believe, is the predominating yellow colouring matter accompanying chlorophyll in the healthy green leaf, and I also find it to be identical with the principal yellow colouring matter of the faded autumnal leaves.

2. Chrysophyll and xanthophyll each give a characteristic absorption spectrum in the violet and ultra-violet region, the former consists of three bands, the latter of four, but in slightly different positious. Crude chlorophyll solutions also give, in addition to the characteristic spectrum of four bands in the less refrangible region, three characteristic bands in the violet, and from observations by means of photography I come to the conclusion that these bands are due to chlorophyll itself, and not to any of the accompanying yellow colouring matters which the majority of former observers believed they were due to. I also find that phyllocyanin and phylloxanthin have bands in identical positions with these three chlorophyll bands.

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* 'Roy. Soc Proc,' vol. 63, p 393.

EXPLANATION OF PLATE.

A and B. Spectrum of a hydrogen vacuum-tube from which the reference lines, F, G′, *h*, and H_1 are obtained, with the potassium K_β line thrown in. The solar lines, L, M, N, and P are obtained by measurement from a negative of the solar spectrum

FIG. 1. Crude chlorophyll in alcohol
,, 2. ,, ,, ,, diluted.
,, 3. Xanthophyll from crude chlorophyll solutions in alcohol by the action of animal charcoal
,, 4. Xanthophyll from faded autumnal yellow leaves in alcohol
,, 5 Chrysophyll in alcohol
,, 6 Crude chlorophyll in alcohol
,, 7. ,, ,, ether.
,, 8 ,, ,, alcohol diluted
,, 9. ,, ,, ether ,,
,, 10. Xanthophyll in alcohol
,, 11. ,, ether.

"On the Chemical Classification of the Stars." By Sir NORMAN LOCKYER, K.C.B., F.R.S. Received April 27,—Read May 4, 1899.

[PLATE 7.]

In the attempts made to classify the stars by means of their spectra, from Rutherford's time to quite recently, the various criteria selected were necessarily for the most part of unknown origin, with the exception of hydrogen, calcium, iron, and carbon, in the main chemical origins could not be assigned with certainty to the spectral lines. Hence the various groups defined by the behaviour of unknown lines were referred to by numbers, and as the views of those employed in the work of classifying differed widely as to the sequence of the phenomena observed, the numerical sequences vary very considerably so that any co-ordination becomes difficult and confusing.

Recent work has thrown such a flood of light on the chemistry of the stars that most definite chemical groupings can now be established, and the object of the present communication is to suggest a general scheme of classification in which they are employed, in relation to the line of cosmical evolution which I have developed in former papers communicated to the Society

The fact that most of the important lines in the photographic region of the stellar spectra have now been traced to their origins renders this step desirable, although many of the chemical elements still remain to be completely investigated from the stellar point of view.

The scheme is based upon a minute inquiry into the varying intensities, in the different stars, of the lines and flutings of the undermentioned substances:—

Certain unknown elements (probably gaseous, unless their lines represent "principal series") in the hottest stars, and the new form of hydrogen discovered by Professor Pickering (which I term "proto-hydrogen" for the sake of clearness), hydrogen, helium, asterium, calcium, magnesium, oxygen, nitrogen, carbon, silicium, iron, titanium, copper, manganese, nickel, chromium, vanadium, strontium, the spectra being observed at the highest available spark temperatures. The lines thus observed I term "enhanced" lines, and I distinguished the kind of vapour which produces them by the affix "proto," *e.g.*, proto-magnesium, for the sake of clearness[*]

Iron, calcium, and manganese at arc temperatures

Carbon (flutings) at arc temperatures

Maganese and iron (flutings) at a still lower temperature

In a communication to the Society[†] I stated the results arrived at recently with regard to the appearances of the lines of the above substances in stars of different temperatures, and the definition of the different groups or genera to be subsequently given are based upon the map which accompanied the paper, together with more minute inquiries on certain additional points, the examination into which was suggested as the work went on

So far as the inquiry has at present gone, the various most salient differences to be taken advantage of for grouping purposes are represented in the following stars, the information being derived from the researches of Professor Pickering[‡] and Mr McClean,[§] as well as from the Kensington series of photographs

Hottest Stars

Two stars in the constellation Argo (ζ Puppis and γ Argûs [||])

Alnitam (ε Orionis) This is a star in the belt of Orion shown on maps as Alnilam Dr Budge has been good enough to make inquiries for me, which show the change of word to have been brought about by a transcriber's error, and that the meaning of the Arabic word is "a belt of spheres or pearls"

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[*] 'Roy. Soc. Proc,' vol 64, p 398

[†] 'Roy. Soc. Proc,' vol 64, p 396

[‡] 'Astro-phys Journ, vol 5, p 92, 1897.

[§] 'Spectra of Southern Stars'

[||] The spectrum of this star contains bright lines, but I show in a paper nearly ready for communication to the Society, that when these occur with dark lines, the latter alone have to be considered for purposes of chemical classification.

Stars of intermediate Temperature.

Ascending Series.	*Descending Series.*
β Crucis.	Achernar.
ζ Tauri.	Algol.
Rigel	Markab
α Cygni.	[ ]
[ ]	Sirius.
Polaris	Procyon
Aldebaran	Arcturus

Stars of lowest Temperature.

Ascending Series.	*Descending Series.*
Antares, one of the brightest stars in Dunér's Catalogue of Class III*a*.*	19 Piscium, one of the brightest stars in Dunér's Catalogue of Class III*b*.
[Nebulæ.]	[Dark Stars.]

In order to make quite clear that both an ascending and a descending series must be taken into account, I give herewith (Plate 7) two photographs showing the phenomena observed on both sides of the temperature curve in reversing layers of stars of nearly equal mean temperatures, as determined by the enhanced lines. The stars in question are :—

Sirius (descending)
α Cygni (ascending).

Procyon (descending).
γ Cygni (ascending).

The main differences to which I wish to draw attention are the very different intensities of the hydrogen lines in Sirius and α Cygni, and the difference in the width and intensities of the proto-metallic and metallic lines in Procyon and γ Cygni. These differences, so significant from a classification point of view, were first indicated in a communication to the Society in 1887†, and the progress of the work on these lines has shown how important they are. I have based the group—or generic—words upon the following considerations.

As we now know beyond all question that a series of geological strata from the most ancient to the most recent brings us in presence of different organic forms, of which the most recent are the most complex, it is natural to suppose that the many sharp changes of spectra observed in a series of stars from the highest temperature to the lowest, bring us in presence of a series of chemical forms which become more complex as the temperature is reduced. Hence we can in the stars study the actual facts relating to the workings of inorganic evolution on lines parallel to those which have already been made available in the case of organic evolution

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* 'Sur les Étoiles à spectres de la troisième classe,'

† 'Roy. Soc. Proc.,' vol. 43, p. 145.

If then we regard the typical stars as the equivalents of the typical strata, such as the Cambrian, Silurian, &c, it is convenient that the form of the words used to define them should be common to both, hence I suggest an adjectival form ending in *ian*. If the typical star is the brightest in a constellation, I use its Arabic name as root, if the typical star is not the brightest, I use the name of the constellation.

The desideratum referred has to a certain extent determined the choice of stars where many were available I have to express my great obligations to Dr Murray for help generously afforded in the consideration of some of the questions thus raised. The table runs as follows.—

Classification of Stars into Genera depending upon their Chemistry and Temperature.

Highest temperature, simplest chemistry.

Argonian

Alnitamian.

Ascending Series	*Descending Series.*
Crucian.	Achernian
Taurian.	Algolian
Rigelian.	Markabian.
Cygnian.	——
——	Sirian.
Polarian.	Procyonian.
Aldebarian.	Arcturian.
Antarian.	Piscian.

The chemical definitions of the various groups or genera are as follows :—

Definitions of Stellar Genera.

Argonian.

Predominant.—Hydrogen and proto-hydrogen.

Fainter.—Helium, unknown gas (λ 4451, 4457), proto-magnesium, proto-calcium, asterium

Alnitamian.

Predominant.—Hydrogen, helium, unknown gases (λ 4089·2, 4116·0, 4649·2).

Fainter.—Asterium, proto-hydrogen, proto-magnesium, proto-calcium, oxygen, nitrogen, carbon.

Crucian. *Predominant.*—Hydrogen, helium, asterium, oxygen, nitrogen, carbon. *Fainter.*—Proto-magnesium, proto-calcium, unknown gas (λ 4089 2), silicium.	***Achernian.*** Same as Crucian.
Proto-metallic lines relatively thick, hydrogen relatively thin.	Proto-metallic lines relatively thin, hydrogen relatively thick.
Taurian. *Predominant.*—Hydrogen, helium, proto-magnesium, asterium. *Fainter.*—Proto-calcium, silicium, nitrogen, carbon, oxygen, proto-iron, proto-titanium.	***Algolian.*** *Predominant.*—Hydrogen, proto-magnesium, proto-calcium, helium, silicium. *Fainter.*—Proto-iron, asterium, carbon, proto-titanium, proto-copper, proto manganese, proto-nickel.
Rigelian. *Predominant.*—Hydrogen, proto calcium, proto-magnesium, helium, silicium. *Fainter.*—Asterium, proto-iron, nitrogen, carbon, proto-titanium.	***Markabian.*** *Predominant.*—Hydrogen, proto calcium, proto-magnesium, silicium. *Fainter.*—Proto iron, helium, asterium, proto-titanium, proto-copper, proto-manganese, proto-nickel, proto-chromium.
Cygnian. *Predominant.*—Hydrogen, proto-calcium, proto magnesium, proto-iron, silicium, proto-titanium, proto-copper, proto chromium. *Fainter.*—Proto-nickel, proto-vanadium, proto manganese, proto-strontium, iron (arc).	***Sirian.*** *Predominant.*—Hydrogen, proto-calcium, proto-magnesium, proto-iron, silicium. *Fainter.*—The lines of the other proto-metals and the arc lines of iron, calcium, and manganese.
Polarian. *Predominant.* — Proto-calcium, proto-titanium, hydrogen, proto-magnesium, proto-iron, and arc lines of calcium, iron, and manganese. *Fainter.*—The other proto-metals and metals occurring in the Sirian genus.	***Procyonian.*** Same as Polarian.

FIG. 1

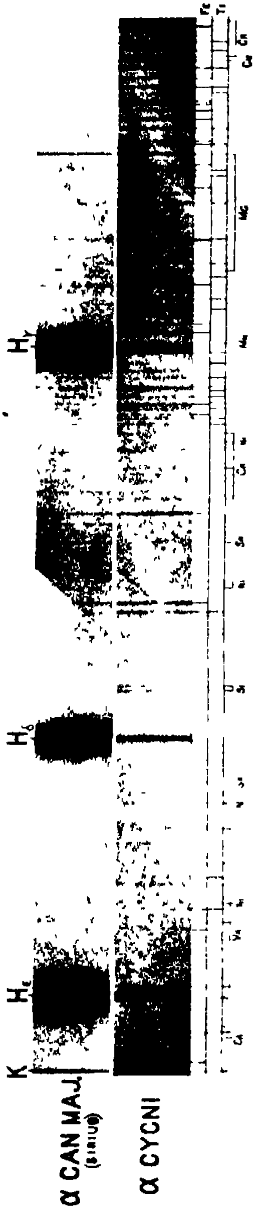
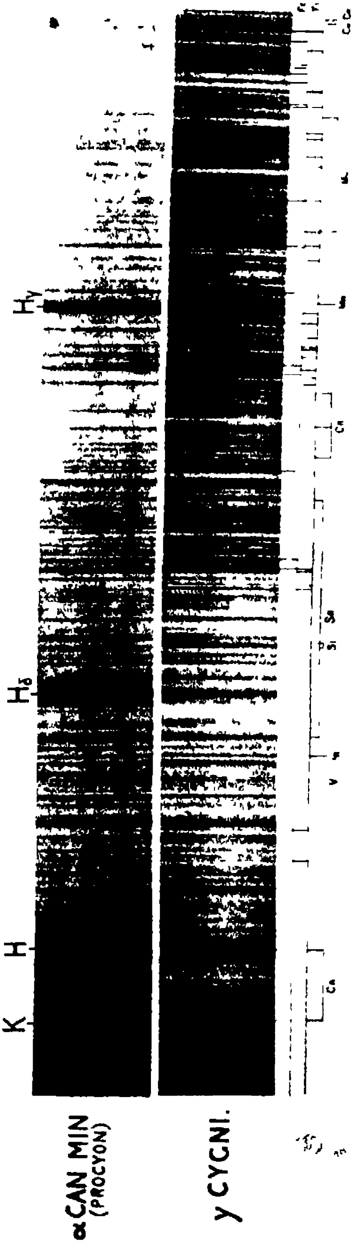

K
Hε
Hδ
Hγ
α CAN MAJ.
(SIRIUS)
α CYGNI
Fe
Ti
Mg

FIG. 2


K
H
Hδ
Hγ
α CAN MIN
(PROCYON)
γ CYGNI.

Aldebarian.

Predominant.—Proto-calcium, arc lines of iron, calcium, and manganese, proto-strontium, hydrogen

Fainter.—Proto-iron and proto-titanium.

Antarian.

Predominant.—Flutings of manganese.

Fainter.—Arc lines of metallic elements.

Arcturian.

Same as Aldebarian

Piscian.

Predominant.—Flutings of carbon.

Fainter.—Arc lines of metallic elements.

We may take for granted that as time goes on new intermediate genera will require to be established; the proposed classification lends itself conveniently to this, as there are no numerical relations to be disturbed.

A still more general chemical classification is the following, it being understood that in it only the most predominant chemical features are considered, and that there is no sharp line of separation between these larger groups. The peculiar position of calcium and magnesium renders this caveat the more necessary

CLASSIFICATION OF STARS.

Highest temperature

Gaseous stars	Proto-hydrogen stars	Argonian Alnitamian	
	Cleveite-gas stars	Crucian. Taurian.	Achernian Algolian
Proto-metallic stars		Rigelian Cygnian ——	Markabian. —— Sirian.
Metallic stars		Polarian. Aldebarian.	Procyonian Arcturian.
Stars with fluted spectra .		Antarian.	Piscian.

Lowest temperature.

The detailed chemical facts to be gathered from the definitions of the several genera indicate many important differences between the order of appearance of the chemical substances in the atmospheres of the stars and that suggested by the hypothetical "periodic law." Special investigations are in progress by which it is hoped some light may be thrown on this and other points of a like nature

"The Diffusion of Ions into Gases." By John S. Townsend, M.A. (Dublin), Clerk Maxwell Student, Cavendish Laboratory, Cambridge. Communicated by Professor J. J. Thomson, F.R.S. Received April 25,—Read May 18, 1899.

(Abstract.)

In the paper upon this subject, the principles upon which the theory of interdiffusion of gases depends, are applied to the diffusion of ions produced by Rontgen rays. When a gas has been left to itself, the conductivity gradually disappears. When no electromotive forces are acting, the loss of conductivity is due partly to positive and negative ions coming into contact with each other, and partly to the effect of the surface of the vessel, which discharges those ions which come into contact with it.

In order to illustrate in a simple way the principles which are involved, we take the case of a gas contained in a metal sphere, and consider what happens to the ions after the gas has been removed from the influence of the rays. For present purposes we may neglect the effect of recombination.

The ions may be considered as constituting a separate gas, the molecules of which may be either bigger or smaller than the molecules of the gas in which they are immersed. When an ion comes into contact with the surface of the sphere it loses its charge, so that the metal may be regarded as a body which completely absorbs the ions. The reduction in the conductivity by the diffusion of the ions to the sides, is exactly analogous to the removal of moisture from a gas by bubbling it through sulphuric acid. The more rapidly the water vapour diffuses through the gas, the greater will be the number of water molecules which come into contact with the acid round the bubble. If the quantity of moisture which is removed be found experimentally, the coefficient of diffusion of water vapour into the gas can be deduced.* It would be impracticable to use this method to find the coefficient of diffusion of ions into a gas contained in a large vessel, as the loss of conductivity due to recombination would be large compared with the loss due to the sides.

The method which was employed was to pass a uniform stream of gas through fine metal tubing, and to allow the rays to fall on the gas immediately before entering the tubing. The bore of the tubing can be so adjusted that the number of ions which come into contact with the sides will be large compared with the number which recombine. It is convenient to use tubing of such a length, that the conductivity will be reduced to about one half its initial value.

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* John S. Townsend, 'Phil. Mag.,' June, 1898.

In order to obtain the coefficient of diffusion when the reduction in the conductivity has been found experimentally, the following problem presents itself :—

If a small quantity of a gas, A, is mixed with another gas, B, and the mixture passed along a tube, the sides of which completely absorb A, to find what quantity of A emerges from the tube with B.

It will be immediately seen that if the gases diffuse rapidly into each other, a large proportion of the molecules of the gas A will come into contact with the surface of the tube and will there be absorbed. If on the other hand the rate of interdiffusion is very small, the molecules of A will travel down the tube in straight lines parallel to the axis of the tube, and practically none of them will come into contact with the surface

The complete solution of the above problem may be obtained from the following equations :—

$$\frac{1}{\kappa}pu = -\frac{dp}{dx}+nXe,$$

$$\frac{1}{\kappa}pv = -\frac{dp}{dy}+nYe,$$

$$\frac{1}{\kappa}pw = -\frac{dp}{dz}+nZe+\frac{1}{\kappa}pW,$$

and the equation of continuity $\frac{d}{dx}(pu)+\frac{d}{dy}(pv)+\frac{d}{dz}(pw)=0$, where n is the number of ions per cubic centimetre, p their partial pressure, e the charge on each ion; X, Y, and Z the electric forces at any point; u, v, and w the velocities of the ions; W the velocity of the gas B through the tube, κ the coefficient of diffusion of the ions into the gas B.

The partial differential coefficient with respect to the time, is omitted from the equation of continuity, as we need only consider the steady state.

The term dp/dz may be omitted from the third equation, as it is small compared with the other terms.

$W=\frac{2V}{a^2}(a^2-r^2)$, where V is the mean velocity of the gas B, defined by the condition πa^2Vt = total volume of gas crossing any section in a time t, a the radius of the tube, and r the perpendicular distance of any point from the axis

The forces X, Y, Z vanish since the electrification is too small to contribute appreciably to the motion of the ions.

The boundary conditions are :—

$$p = 0 \text{ when } r = a,$$

$$p = \text{constant when } z = 0,$$

The solution of the problem requires a somewhat lengthy analysis, so that we give here only the final result.

The ratio of the number of ions (or molecules of the gas A) coming out of the tube with B to the number which enter is

$$R = 4\left[0·1952\,\epsilon^{-\frac{7·313\kappa z}{2a^2V}} + 0·0243\,\epsilon^{-\frac{44·5\kappa z}{2a^2V}} + \&c.\right],$$

where z is the length of the tube.

The other terms of the series are too small to be taken into consideration.

Having determined the reduction in conductivity, due to tubes of different lengths, the following values of the coefficients of diffusion of ions into air, oxygen, carbonic acid, and hydrogen, were obtained.

Table of Coefficients of Diffusion of Ions in dry Gases.

Gas	κ for + ions.	κ for − ions.	Mean value of κ.	Ratio of the values of κ.
Air	0·0274	0·042	0·0347	1·54
Oxygen	0·025	0·0396	0·0323	1·58
Carbonic acid	0·023	0·026	0·0245	1·13
Hydrogen	0·123	0·190	0·156	1·54

Table of Coefficients of Diffusion of Ions in moist Gases.

Gas.	κ for + ions	κ for − ions	Mean value of κ.	Ratio of the values of κ.
Air	0·032	0·035	0·0335	1·09
Oxygen	0·0288	0·0358	0·0323	1·24
Carbonic acid.... ...	0·0245	0·0255	0·025	1·04
Hydrogen	0·128	0·142	0·1350	1·11

We should expect from the experiments, that the above numbers were correct to 5 per cent.

Considering one of the equations of motion

$$\frac{1}{\kappa}pu = -\frac{dp}{dx} + nXe,$$

we see that when $dp/dx = 0$, the velocity u due to the electric force X is $nXe\kappa/p$ If the potential gradient is one volt per centimetre $X = 1/300$ in electrostatic units, and the corresponding value of u is

$$u_1 = \frac{\kappa e}{300} \cdot \frac{n}{p}.$$

Let N be the number of molecules in a cubic centimetre of a gas at pressure P, equal to the atmospheric pressure, and temperature 15° centigrade, the temperature at which u and κ were determined.

The quotient N/P may be substituted for n/p in the above equation, and since the atmospheric pressure P is 10^6 in C.G.S units, we obtain

$$Ne = \frac{3 \times 10^8 u_1}{\kappa}.$$

The following values of Ne thus obtained for different gases are

Air	$Ne_A = 1{\cdot}35 \times 10^{10}$
Oxygen	$Ne_O = 1{\cdot}25 \times 10^{10}$
Carbonic acid	$Ne_C = 1{\cdot}30 \times 10^{10}$
Hydrogen	$Ne_H = 1{\cdot}00 \times 10^{10}$

The values of u were taken from the table of mean velocities given by Professor Rutherford.[*] The values of κ which were used, are the mean values obtained for dry gases.

Experiments on electrolysis show that one electrodynamic unit of electricity in passing through an electrolyte, gives off 1·23 c.c. of hydrogen at temperatures 15° and pressure 10^6 C.G.S units. The number of atoms in this volume is 2·46 N, so that if E is the charge on an atom of hydrogen in the liquid electrolyte,

$$\begin{aligned} 2{\cdot}46\text{NE} &= 1 \text{ electrodynamic unit of quantity} \\ &= 3 \times 10^{10} \text{ electrostatic units.} \end{aligned}$$

Hence $$\text{NE} = 1{\cdot}22 \times 10^{10},$$

the charge E being expressed in electrostatic units.

Since N is a constant, we conclude that the charges on the ions produced by Röntgen rays in air, oxygen, carbonic acid, and hydrogen, are all the same, and equal to the charge on the hydrogen ion in a liquid electrolyte.

Professor Thomson[†] has shown that the charge on the ions in hydrogen and oxygen which have been made conductors by Röntgen rays, is 6×10^{-10} electrostatic unit, and is the same for both gases.

Taking this value for the charge e, we obtain the number of molecules in a cubic centimetre of a gas

$$\text{N} = 2 \times 10^{19}.$$

From this we deduce the weight of a molecule of hydrogen, ρ/N,

$$4{\cdot}5 \times 10^{-24} \text{ gram.}$$

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[*] E. Rutherford, 'Phil. Mag.,' November, 1897.

[†] J. J. Thomson, 'Phil. Mag.,' December, 1898.

Since, as we have shown, the charge on an ion produced by Röntgen rays is equal to the charge on a hydrogen ion in a liquid electrolyte, this latter charge is also 6×10^{-10} electrostatic unit.

Although the value of Ne for hydrogen is 25 per cent. less than its value for other gases, we are justified in including hydrogen in the above general conclusion, as we should expect the value of u for hydrogen to be too small. Professor Rutherford makes no mention of having corrected for the presence of air in his apparatus, or of having used perfectly dry hydrogen. If we take the mean value of κ for moist hydrogen, we obtain $Ne_{H} = 1·15 \times 10^{10}$

In order to prove that the charge on the positive ion is equal to the charge on the negative ion, the ratio of the coefficients of diffusion must be shown to be equal to the ratio of the velocities Professor Zeleny* has shown that the negative ions travel faster under an electromotive force than the positive ions, the ratios of the velocities being 1·24 for air and oxygen, 1·15 for hydrogen, and 1·0 for carbonic acid.

The experiments on diffusion show that the ratio of the velocities would be larger in dry than in moist gases, but as this point has not yet been examined by Professor Zeleny, we cannot expect a very close agreement between the ratios which he gives for the velocities and the ratios of the coefficients of diffusion.

We are led to conclude that the charges on the positive and negative ions are equal from another point of view. It has been proved that the mean charge is the same as the charge on an ion of hydrogen in a liquid electrolyte If the charges differed, one of them would be less than the charge on the hydrogen ion, whereas experiments on electrolysis show that all ionic charges are either equal to the charge on the hydrogen ion or an exact multiple of it.

"On the Presence of Oxygen in the Atmospheres of certain Fixed Stars." By DAVID GILL, C.B, F.R.S, &c., Her Majesty's Astronomer at the Cape of Good Hope. Received April 14,—Read April 27, 1899.

(PLATE 8)

In a paper read before the Society on April 8, 1897, and in a subsequent paper,† Mr. Frank McClean draws attention to the grouping of lines other than those of helium and hydrogen in the spectra of β Scorpii, β Canis Majoris, β Centauri and β Crucis, suggesting that the close correspondence between the grouping of these extra lines and the known lines of oxygen, points to the probable presence of that gas in the atmospheres of these stars.

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* J. Zeleny, 'Phil. Mag.,' July, 1898.

† 'Roy. Soc. Proc.,' vol. 42, No. 386, p. 418.

In the latter paper he writes, "The most remarkable correspondence is in the case of the large group on either side of H_δ. A slight shift of about a tenth-metre is required to bring the groups into identical positions. However, the close similarity of the whole grouping of the two spectra as they appear on the plate, admits of little doubt that the extra lines actually constitute the spectrum of oxygen If this be established, the spectrum of the first division of helium stars would be due to hydrogen, helium, and oxygen"

In his subsequent work* Mr. McClean concludes, "Taking everything into account, the succession of coincidences between the extra lines of β Crucis and the oxygen spectrum can only be accounted for on the basis of the extra lines being in the main actually due to oxygen."

This conclusion does not, as yet, appear to have been fully accepted by spectroscopists, partly because, from the low dispersion used, the lines of the groups are not separately shown. It is very generally known that the instrumental equipment of the Royal Observatory at the Cape, has recently been enriched by a complete equipment for Astrophysical research, the whole being the munificent gift of Mr McClean, F.R S

The slit spectroscope, for attachment to the photographic refractor, reached the Cape in the middle of January last, and I resolved that its first published work should deal with Mr. McClean's interesting discovery.

As a complete account of the instrument and its observatory will be subsequently published, it may be sufficient for the present to state that the object glass of the photographic telescope has an aperture of 24 inches and focal length of 22 feet 6 inches, its minimum focus being, at present, for rays about midway between H_β and H_γ.

The collimator of the spectroscope has an aperture of 2¼ inches and focal length of 22½ inches, so that a cylinder of parallel rays 2 inches in diameter falls on the prisms, and the latter are of sufficient size to pass the whole of the rays which form the image of the spectrum on the sensitive plate. The instrument is provided with two camera-telescopes of 2¾ inches aperture, one being of about 36 inches focal length, the other of 16 inches

Only the larger of the two camera-telescopes have been employed in the after-mentioned observations

There are two cast-iron prism boxes; one of them contains three prisms of about 60° each, which for rays near H_γ produces a deviation of 180°, so that the camera-telescope becomes parallel in the reverse direction to the slit-telescope. The other prism box contains a single prism of 62°. The prisms in both boxes are fixed, without screw adjustment, in minimum deviation for H_γ. The collimator is in the axis of a solid drawn steel cylinder—the latter attaching by a flange at one end to the butt end of the telescope, a cast-iron plate attaching to the flange on the other end of the cylinder carries either one or other of the two prism boxes.

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* 'Spectra of Southern Stars' (Stanford, London, 1898).

The slit-slide and 60° prism for reflecting the comparison spark on the slit (made on the plan of the Lick spectroscope) are contained in a strong cast-steel box which is permanently attached to one end of the collimator tube. This latter is a very strong solid drawn steel tube, the external surface of which has been turned and finished to a true cylindrical form, and it rests in proper geometrical bearings formed in strong cast-iron diaphragms, which are fitted inside the cylindrical body of the instrument.

A powerful slow motion permits the collimator to be slid along its axis so as to focus the slit upon the image of the star at any required reading of the focussing scale. The object-glass of the collimator is mounted on the end of another steel cylinder which also rests on geometrical bearings inside the outer collimator tube, and it is also provided with fine slow motion and a focussing scale Both these scales are illuminated at will by small incandescent lamps, and are read by microscopes which are accessible from the outside.

The whole instrument can be enveloped in felt to prevent any but very slow change of temperature

The comparison-spark apparatus is arranged with wide angle object-glasses, in such a way that if the image of the spark shines on the slit the object-glass of the collimator must be full of light. Numerous trials in all positions of the instrument have invarably given photographs of the lines of the comparison spectrum of iron rigorously coincident with the corresponding lines of the solar spectrum, the latter being obtained by exposing the slit in diffuse daylight.

The camera end with its focussing and tilting adjustments can be attached to either telescope by a flange with bayonet joint. The focussing scale is divided to 1/10 mm., and the amount of tilt of the plate-holder is measured on a graduated arc.

As the large telescope is fitted with an object-glass prism of 24 inches aperture (which, when the slit spectroscope is in use, is folded back in the manner shown in the frontispiece of Mr. McClean's 'Spectra of Southern Stars'), heavy counterpoises are required to balance the tube about the declination axis if the spectroscope is not attached.

In designing the slit spectroscope I was thus not limited by the necessity for lightness in its construction. The complete instrument weighs 400 lbs., being almost exactly the equivalent of the counterpoises and focussing slide and camera, which are removed for its adaptation. In every detail the fittings of the spectroscope are designed with the necessary geometrical limitations of freedom *and no more*, so that no shake nor variation of adjustment can arise from imperfection of workmanship; that is to say, all the adjustments depend on adequate spring pressure against the *minimum number* of necessary rigid points of support.

The object-glasses of the spectroscope were made by Brashier, and are all excellent. The three dense prisms were also made by Brashier, their definition is very fine, but the glass is rather yellow in colour, and produces great absorption of rays more refrangible than H_γ The single prism, by Steinheil, gives excellent definition, and the glass is much whiter than in Brashier's prisms. The optical constants of the prisms have not yet been determined.

The mounting was constructed by the Cambridge Scientific Instrument Company to my designs, in the most careful and satisfactory way. I am greatly indebted to Mr. Horace Darwin for much care in supervision of the work and for some very ingenious and important improvements in detail which he carried out.

Above all I am indebted to Mr. H. F Newall, who has taken infinite trouble in making and testing the permanent adjustments of the instrument and in supervising the arrangement of its final details. To him I owe the fact that the instrument arrived at the Cape practically in perfect adjustment and ready for work After a series of preliminary focussing trials by Newall's method,* a number of photographs of star spectra were made with the three-prism box and long telescope The present paper deals chiefly with the results of measures of a photograph of the spectrum of β Crucis and of a comparison iron spectrum obtained on March 15. The plate was exposed to the comparison spectrum of iron immediately before and immediately after the exposure for the star spectrum (See Plate 8)

Lines of the iron spectrum cover the whole exposed length of the plate from Fe λ 4187·99 to 4563·99, the linear interval on the plate between these lines being 70 367 mm.

As a preliminary step, the intervals between successive pairs of iron lines were measured with the micrometer of the old Repsold astro-photographic measuring apparatus.† If

Δs is the interval between two adjoining lines in terms of revolutions of the micrometer screw,

λ_1 and λ_2 their respective wave-lengths,

$\lambda_m = \frac{1}{2}(\lambda_1 + \lambda_2)$ and $\Delta\lambda = \lambda_1 - \lambda_2$,

I found, to my surprise, on computing $\Delta\lambda/\Delta s$ for many different values of λ_m, and plotting these on millimetre paper with $\Delta\lambda/\Delta s$ as ordinate and λ_m as abscissa, that within the limits of error of plotting and observation the resulting curve was practically a straight line : in other words, the screw values can be represented by

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* 'Monthly Notices, R.A.S ,' vol. 57, p. 572.

† Described by Bakhuyzen, 'Bulletin du Congrès Astrographique,' vol. 1, p. 169.

$$\frac{\Delta\lambda_\lambda}{\Delta s} = n,$$

$$\frac{\Delta\lambda_{\lambda+1}}{\Delta s} = n + d, \qquad \frac{\Delta\lambda_{\lambda-1}}{\Delta s} = n - d,$$

$$\frac{\Delta\lambda_{\lambda+2}}{\Delta s} = n + 2d, \qquad \frac{\Delta\lambda_{\lambda-2}}{\Delta s} = n - 2d,$$

or generally

$$\frac{\Delta\lambda}{\Delta s} = d(\lambda_\lambda - \lambda_0)$$

where

$$\lambda_0 = \lambda_\lambda - n/d.$$

Such a law can only be applicable to a limited portion of the whole spectrum, for it is obvious that for no value of λ can $\Delta\lambda/\Delta s$ be really $= 0$

In order to test over what range of the spectrum this law might be regarded as sufficiently rigorous, four selected iron lines were measured in terms of the millimetre scale which is attached to the instrument, the division errors of which are known for each 5th millimetre For convenience the measures are converted into screw revolutions of the micrometer microscope which was used in measuring the spectra, viz., 1 revolution = 0·5 mm

			Screw Revs
λ_1 =	Fe	4583·99	200·254
λ_2 =	,,	4442·52	159·130
λ_3 =	,,	4282·54	101·802
λ_4 =	,,	4187·99	59·520

Hence,

$$\begin{array}{llcc}
 & & d. & \lambda_0 \\
\text{for } \frac{\lambda_1+\lambda_2}{2} \text{ we have} & \frac{\Delta\lambda_{4513\cdot26}}{\Delta s} = 3\cdot1400 & & \\
 & & 0\cdot0043053 & 3714\cdot80 \\
\text{,,} \quad \frac{\lambda_2+\lambda_3}{2} \quad \text{,,} & \frac{\Delta\lambda_{4362\cdot53}}{\Delta s} = 2\cdot7906 & & \\
 & & 0\cdot0043572 & 3722\cdot08 \\
\text{,,} \quad \frac{\lambda_3+\lambda_4}{2} \quad \text{,,} & \frac{\Delta\lambda_{4235\cdot27}}{\Delta s} = 2\cdot2361 & &
\end{array}$$

Thus for interpolation between adjoining known iron lines the second differences, d, are practically constant over the range of spectrum with which we have to deal, and consequently the logs of $(\lambda - \lambda_0)$ vary proportionately to the measured intervals between the lines.

If this were *strictly* true for the whole range of our spectrum one would obtain rigorously accurate interpolation as follows.—

Let m_1 and m_3 be the micrometer readings on any two known lines,
" m_2 " " " for an intermediate unknown line,
" λ_1 and λ_3 the corresponding wave-lengths of the known lines,

then to find λ_2 corresponding to m_2 we have

$$([\lambda_1 - \lambda_0] - [\lambda_3 - \lambda_0])\frac{m_3 - m_2}{m_3 - m_1} + [\lambda_3] = [\lambda_2 - \lambda_0] \ldots\ldots\ldots \quad (1),$$

where the square brackets denote the logs of the included quantities.

That the second differences of $\Delta\lambda/\Delta s$ for successive values of λ are not strictly constant for the whole length of our spectrum is shown by the different values of d and λ_0 obtained above.

If we assume λ_0 a constant = 3718 and interpolate the wave-lengths of λ_2 and λ_3, by means of our formula (1) we obtain

	Known	Computed	Known – Computed
λ_2	4425·52	4425·36	+0·16
λ_3	4282·54	4282·72	–0·18

The differences "known – computed" considerably exceed the probable errors due to the observations and the small inaccuracies of the determinations of the fundamental wave-lengths, thus showing that errors have been introduced by neglect of the small variations in the second differences of the successive values of $\Delta\lambda/\Delta s$.

It is however very easy to take account of these variations by computing an auxiliary table for different values of λ_0 with the argument λ_1. Thus, on the assumption that λ_0 varies proportionally to λ_1 we have.—

Mg. λ	λ_0
4100	3732
4200	3727
4300	3722
4400	3717
4500	3712
4600	3707

Taking λ_0 from this table with the argument 'approximate wave-length of the line, whose definitive wave-length is required,' we find the intermediate wave-lengths accurately represented.*

As well known iron lines are found within 30 or 40 tenth-metres of all the unknown lines, it is always sufficient for the purpose of interpolation by our formula to employ the value of λ_0 from the above table with the argument $\frac{\lambda_1 - \lambda_3}{2}$

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* I find that the wave-lengths of solar lines as measured by Campbell ('A. P. Journal,' October, 1898) are also very beautifully represented throughout by this simple method of interpolation.

In this way the wave-lengths of the lines in the spectrum of β Crucis, given in the following table (pp. 203—204), have been determined:—

Column (1) gives the wave-lengths of all the known oxygen lines of intensity 3 or brighter, between λ 4303 and 4575, according to Neovius as well as Trowbridge and Hutchins.*

Column (2) gives the wave-lengths of all helium lines according to Runge and Paschen,† contained within limits of the spectrum under observation.

Column (3) gives the results of my measurements of the negative of the spectrum of β Crucis, those under head I being my first essays in the measurement of any photographed spectrum, those under head II being the results of my second measurement, including all the lines which could be detected under most careful and repeated scrutiny. Each result in series I depends on two pointings, each in series II on four pointings.

The lines whose wave-lengths are given to two decimal places of the tenth-metre, were measured with a magnifying power of fifteen diameters, those give to one decimal place with a power of only three diameters —the lines of the latter class being very faint and only certainly visible under a very low power. When possible, different iron lines were used in series I and II for determination of the wave-lengths of the stellar lines.

The observations were not arranged for determination of motion in the line of sight, but the exact coincidence of the star line 4417·06 with the air (oxygen) line, and the general agreement of the stellar hydrogen and helium lines with their known wave-lengths, tend to show that the relative motion of β Crucis to the Earth on February 21 did not exceed ± 3 kilometres per second. On that date the Earth in its motion round the Sun was moving towards β Crucis with a velocity of 18 kilometres per second, and consequently β Crucis is probably receding from the Sun with a velocity of 18 kilometres ± 3 kilometres per second.

The whole of the known helium lines within the measured range of spectrum are unquestionably present, as also are all known oxygen lines stronger than intensity 4.

The exceedingly faint lines:—

β Crucis 4253·9 may be coincident with Neovius 4254·1 = T. and H. 4253·42, and

β Crucis 4303·0 or 4304·0 may be coincident with Neovius 4304·4 = T. and H. 4303·8,

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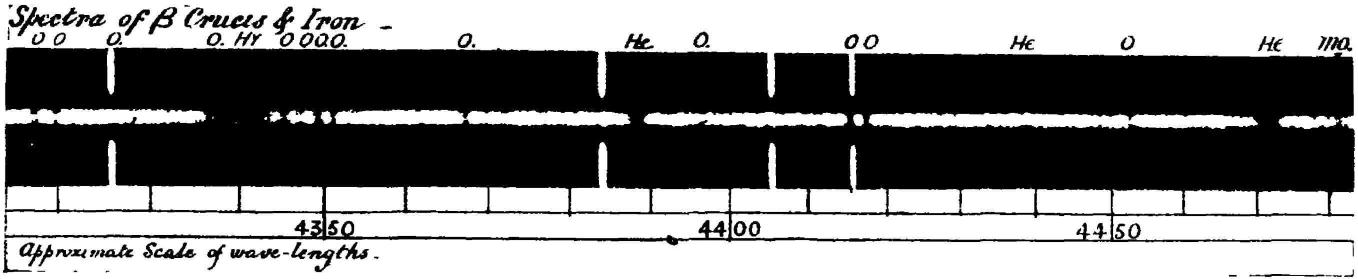
* Watts, 'Index of Spectra,' Appendix E.

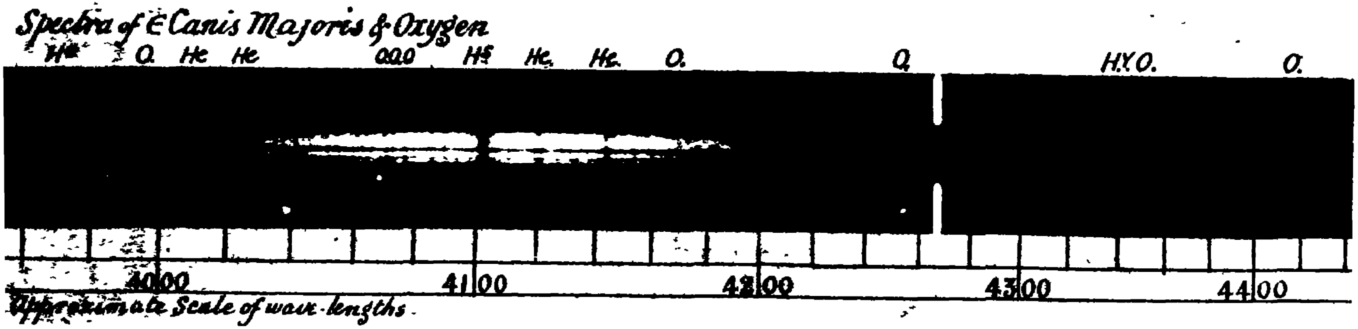
† 'A. P. Journal,' vol. 3, p. 10.

(1) Known oxygen lines. Intensity 3 or brighter.			(2)	(3) Cape measures of spectrum of β Crucis.		(4)
Neovius.	Intensity.	Trowbridge and Hutchins	Helium (Runge and Paschen, 'A. P. Journal,' vol 3 p 10).	I	II	Remarks
					4253 9	Faint, rather broad, edges fairly distinct.
					4267 2	Probably carbon, E and V, 4267 5
					4285 3	Undoubted line, only seen with low power
					4298 0	Too indistinct for measurement, positions estimated only
					4303 0	
					4304 0	
4317·4	(4)	4317 20		4317·36	4317 35	O
4319·9	(4)	4319 50		4319 78	4319 78	O.
4325·9	(3)	4325 90		4325 83	4326 0	O
4327 3	(3)	4327 60				
4337·1	(3)			4337·19	4337 0	O slightly more refraction than Fe 4337·22.
					4338 8	
				4340·57	4340 63	H$_\gamma$ (4340·634) Rowland
					4342 6	Suspect this faint close double line.
4345·8	(6)	4345 52		4345 53	4345 50	O
4347·9	(6)	4347 94 4347 47		4347 33	4347·30	O coincident with 4347·47.
4349 4	(8)	4349·30		4349 42	4349·49	O
4351 6	(6)	4351·40		4351·34	4351 39	O
					4361·4	Both very fine faint lines very difficult to see
					4363·1	
4367·0	(6)	4366·92		4367·01	4366·99	O.
4368·3	(3)					
4369 7	(3)	4369·60				
			4388 100 (3)	4388 18	4388 24	He.
4396·1	(3n)	4396 30			4396 2	O

(1.) Known oxygen lines Intensity 3 or brighter.			(2) Helium (Runge and Paschen, 'A P. Journal,' vol 3, p 10)	(3.) Cape measures of spectrum of β Crucis.		(4) Remarks
Neovius	Intensity	Trowbridge and Hutchins		I	II	
4415·0	(9)	4515 00		4415 01	4415·09	O
4417·3	(9)	4417 17		4417 06	in coinc with air line	O
					4431 1	Certainly less refraction than Fe 4430·79.
					4433 1	Certainly more refraction than Fe 4433 32.
					4435 1	
			4437 718 (1)		4437 9	He faint, well defined edges
4452 7	(3)	4452 40			4452 7	O.
4465·4	(4)	4465·40				
4467·8	(4)	4468 04				
4469 6	(3n)	4469·50				
			4471 646 (6)	4471·61	4471 56	He
					4481·17	Probably magnesium
					4522·84	Very faint.
				4552 80	4552·78	Strong well marked lines, origin unknown.
				4567·90	4568·06	
				4574 67	4574 69	


Spectra of β Crucis & Iron
O O
O.
O. Hγ
O O O O.
O.
He
O.
O O
He
O
He
Mg.
43|50
44|00
44|50
Approximate Scale of wave-lengths.

1. Spectra of star β Crucis and of Iron near Hγ


Spectra of ε Canis Majoris & Oxygen
Hε
O.
He
He
O O O
Hδ
He.
He.
O.
O.
H.γ O.
O.
40|00
41|00
42|00
43|00
44|00
Approximate Scale of wave-lengths.

2. Spectra of ε Canis Majoris and Oxygen near Hδ

In upper figure line at 4481.2 should be Mg (magnesium.)

but these coincidences are very doubtful, and it is improbable that such very faint lines would be represented, whilst the neighbouring line 4327·3, intensity 3, is wanting on the photograph.

The oxygen lines of intensity 4 which are wanting, viz.—Neovius 4465·4 and 4467·8 are in a portion of the spectrum which is somewhat over exposed, and this fact probably accounts for the non-appearance on the plate. Possibly also the relative intensities of the oxygen lines at the temperature and pressure of the atmosphere of β Crucis may be different from their relative intensities in the conditions under which Neovius determined the intensities of the air lines (spark spectrum).

There remains however not the slightest doubt, that all the stronger oxygen lines are present in the spectrum of β Crucis, at least between λ 4250 and 4575, and this fact requires no further laboratory experiments for its establishment. It is almost equally certain that there is no trace of true nitrogen lines in this spectrum.

The only measured lines of β Crucis near known nitrogen lines are:—

Neovius	4341·8	Intensity	1*b*	β Crucis	4342·6
„	4523·0	„	1*b*	„	4522·84

but it is improbable, although not impossible, that nitrogen lines of intensity (1) should be present whilst the strong nitrogen line 4507·7, of intensity 6, is absent.

Besides hydrogen, helium and oxygen, the spectrum of β Crucis shows the probable presence of carbon (4267·2) and magnesium (4481·17). These lines were not included in the first measurement, in the former case because the line could not be distinctly seen with the higher power, in the latter because I was doubtful of the existence of the line on account of a slight defect in the film at that point, but other negatives confirm its existence. A contact positive from the original negative of the spectrum is sent herewith for reproduction.

The spectra of β Crucis, β and ϵ Canis Majoris, and probably β Centauri are all practically identical. They all contain the three unknown strong lines

4552·79
4567·09
4574·68

besides the probable magnesium line 4481·17, the lines of hydrogen, helium, the stronger oxygen lines, and the probable carbon line 4267·2.

Farther investigations on this class of stars will be subsequently communicated; in the meanwhile I forward also for reproduction a contact positive from a negative of the spectrum of ϵ Canis Majoris taken on March 15 with the single prism, in which the slit has been focussed for rays of about λ 4080, and with a comparison spectrum from an oxygen tube, a leyden jar and air space being introduced in the secondary circuit of the Ruhmkorff coil. (See Plate 8.)

This photograph shows the coincidence of stellar lines with the group of three strong oxygen lines, viz :—

Ncovius	T and H.	Intensity
4076·3	4076·19	9
4072·4	4072·34	9
4070·1	4070·24	8

and other neighbouring oxygen lines beyond the range of the three prism train. The lines are all displaced towards the red by motion.

On March 15 the Earth by its motion round the Sun was receding from ε Canis Minoris, with a velocity of 17 kilometres per second, which agrees with the direction of the displacement of the oxygen lines. There has not yet been time to determine the constants of the single prism spectroscope, but this does not affect the question of the identification of the oxygen lines in the spectrum of ε Canis Majoris.

The plates of the spectra of *β* Crucis and ε Canis Majoris were exposed and developed by my assistant, Mr. J. Lunt.

June 1, 1899.

Annual Meeting for the Election of Fellows.

Professor T. G. BONNEY, Vice-President, in the Chair.

The Statutes relating to the Election of Fellows having been read, Dr. A. A. Common and Dr. J. H. Gladstone were, with the consent of the Society, nominated Scrutators, to assist the Secretaries in the examination of the balloting lists.

The votes of the Fellows present were collected, and the following Candidates were declared duly elected into the Society :—

Barrett, Professor William
Booth, Charles, D.Sc.
Bruce, David, Major R.A.M.C.
Fenton, Henry John Horstman, M.A.
Gamble, James Sykes, M.A.
Haddon, Professor Alfred Cort, M.A.
Head, Henry, M.D.
Hele-Shaw, Professor Henry Selby, M.Inst.C.E.
Morgan, Professor Conwy Lloyd, F.G.S.
Reid, Clement, F.G.S.
Starling, Ernest Henry, M.D.
Tanner, Professor Henry William Lloyd, M.A.
Threlfall, Richard, M.A.
Tutton, Alfred E., B.Sc.
Windle, Professor Bertram Coghill Allen, M.D.

Thanks were given to the Scrutators.

June 1, 1899

Professor T. G BONNEY, Vice-President, in the Chair

A List of the Presents received was laid on the table, and thanks ordered for them.

Professor Ludwig Boltzmann, Professor Anton Dohrn, Professor Emil Fischer, Dr Georg Neumayer, and Dr. Melchior Treub, were balloted for and elected Foreign Members of the Society.

The following Papers were read —

I "The Parent-rock of the Diamond in South Africa" By Professor T. G. BONNEY, F.R.S.

II "Experimental Contributions to the Theory of Heredity. A. Telegony." By Professor J. C. EWART, F.R S.

"The Characteristic of Nerve" By AUGUSTUS D. WALLER, M D, F.R S. Received February 3,—Read February 16, 1899.

The object of the present preliminary series of experiments was to determine whether the excitability (or, as I should prefer to say in this connection, the mobility) of living matter can be gauged by the rate of impact of a mobilising stimulus, in other words, whether, for various kinds of more or less mobile protoplasm, an optimum stimulation-gradient can be found, above and below which the curve of stimulation is less perfectly adapted to the movement caused by stimulation. In, *e.g.*, the case of the rolling of a ship, there is an optimum wave-length and wave-face outside the limits of which the "mobilisation" is less than maximal. And just as the optimum wave-length giving greatest movement by least energy harmonises with and gives therefore measure of the oscillation-period of a particular ship, so the optimum gradient of mobilising energy producing greatest excitation by least energy, might be expected to give measure of the excitation-period proper to a particular tissue, *and to characterise that tissue.*

The best instrument at our disposal for an examination of this point is obviously a condenser, of varied capacity, charged at varied pressure. This method of excitation has been put into effect by several previous observers. Chauveau (1) first systematically studied the "law of contractions," and demonstrated upon frog's nerve that the make excitation is kathodic. Dubois (4, 5), of Bern, studied the law of contractions upon human nerve, and came to the conclusion that the greater and smaller effects of condenser discharges are governed by the greater and smaller *quantities* of electricity in play. Hoorweg (11, 12), Cybulsky and Zanietowsky (9, 10) studied the effects more closely upon frog's nerve; the first-named observer concluded that the effects are a function of *intensity*, while the last two observers, from very similar observations, concluded that the magnitude of excitation is a function of *energy*. Salomonsen (8), Boudet (2), and D'Arsonval (6, 7) come to the same conclusion, and the last-named observer makes use of an expression—"la caractéristique de l'excitation"—very similar to the designation that I had been independently led to adopt, in ignorance of its previous use in a different sense.

The problem is, I think, to be considered from the following *a priori* point of departure.—A stimulus arouses excitable matter by reason of its actual energy and not of its mere quantity. A weight *per se* is not a stimulus, but a weight dropped from a height acts as a stimulus that is to be expressed in terms of energy. One gram fallen through 1 cm. strikes and stimulates with an actual energy of 1000 (or more precisely 981) ergs. Similarly, as regards the electrical stimuli afforded by the (charge or) discharge of a condenser, it is neither the quantity (coulomb) nor the pressure (volt) that alone gives measure of the stimulus, but the energy (Joule or 10^7 ergs) of that quantity at that pressure. A nerve (or other excitable tissue) may be struck and stimulated by an electrical energy of so many ergs. And we are adequately acquainted with the physiological value of a stimulus when we know.—First, its absolute value in ergs or fraction of an erg. Second, the rate at which such energy impinges upon and sets in motion the excitable molecules under investigation.

The absolute value of a true minimal stimulus in ergs or fraction of an erg, is ascertained by determining the optimum capacity and voltage at which a minimal response is obtained. The energy E in ergs $= 5\,FV^2$, where F is in microfarads and V in volts.

Its rate of impact depends upon the rate of (charge or) discharge $= \frac{V}{FR} \times$ a constant, where R denotes the resistance in circuit in ohms, and can therefore be expressed by a number that will be higher or lower according as the rate of discharge is greater or smaller.*

---

* With the same units as above, and with the unit of time $= 10^{-6}$ second, the constant is 0·8687. In the subjoined data it has been taken as 868700 for the unit of time = 1 second.

For certain ends it is convenient to indicate the rate of discharge of energy by stating the time necessary for its fall to any given fraction $1/n$ of its original value, according to the formula $t = FR\frac{\log n}{2 \log e}$; *e.g.*, with a capacity of 0·01 microfarad, The number of the true minimal stimulus at optimum capacity and voltage, is what I propose to designate as the characteristic, or otherwise stated—the characteristic is the constant of the curve of the smallest discharge of energy that can provoke movement, or of the discharge of energy provoking greatest movement.

By suitable adjustment of capacity and voltage we may obtain the discharge of any desired energy—at any desired rate, if we also know the resistance through which discharge is made From a small condenser at high voltage, and from a large condenser at low voltage, we may obtain the discharge (of equal quantities or) of equal energies, but in the first case rapidly and in the second case slowly. We may so adjust capacity and voltage, as to deliver constant energy in a series of curves of varying steepness, and find by experiment the more or less precise limits of steepness between which the motion or excitation is greatest The number denoting that steepness of energy discharge by which a nerve is most economically mobilised, signifies the optimum adaptation of excitation to excitability, and is its characteristic At that number the nerve is excited by the minimum of energy With a stimulus of higher or lower number, more energy is necessary to produce an equal effect

Taking either a series of stimuli of given energy, but of varying gradients, as in experiments 1 and 9, we are to observe at what gradient the maximum effect is produced Or as in experiment 2, making a series of trials with varying amounts of energy from varying capacity and voltage, we are to observe at what minimum energy discharge the smallest perceptible effect is produced, and thence calculate the characteristic.

And even without actually determining the characteristic by means of its upper and lower limits, we may often usefully ascertain whether it is above or below a lowest or highest constant at the end of a series of experimental data Thus in experiment 6, a minimal energy has not been reached, and we cannot therefore say that the characteristic has been determined, we know however, that it is less than 10·4. In experiment 4 the characteristic, as defined above, has not been actually 11382 and 0·6 since the stimulus has not been truly minimal; but these values have been those of the constants of suitable stimuli not far from minimal, and we know therefore that the characteristic has been nearer to the higher value at high temperature, nearer to the lower value at low temperature

---

a resistance of 100,000 ohms, and $1/n = \frac{1}{2}$, the time of discharge of $\frac{1}{2}$ of the original energy is 0·000693 sec. If the voltage under which this discharge takes place is 0·2, the original energy is 0·002 erg, and the number indicating its rate of discharge is 174. If 0·01 mf. and 0·2 v should be the optimum capacity and voltage of a minimal effect by excitation of a given nerve, *i.e.*, a true minimal or optimal minimal stimulus, then the number 174 is the characteristic of the nerve.

Strictly speaking, the term minimal stimulus in, *e.g.*, experiment 2, applies only to the two values 0·001134 erg and 0·002 erg in the two groups respectively, since in these two cases only is the energy of the stimulus at its minimum.

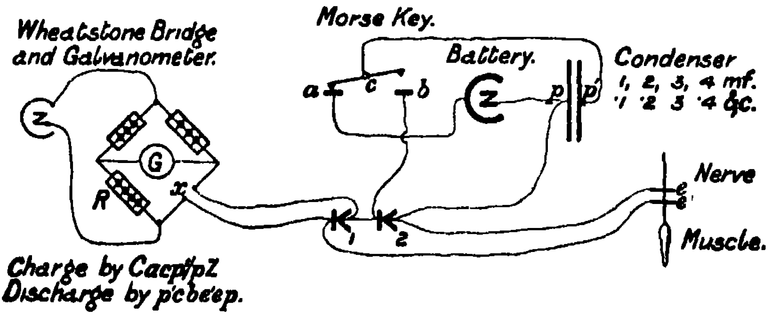
But in accordance with ordinary tests by induction currents, the term "minimal" is applied to stimuli that are certainly not of minimal energy, but that produce minimal effects And in this loose sense all the trials of the above groups give minimals, in the first group minimals of effective capacity at the several voltages, in the second group minimals of effective voltage at the several capacities.

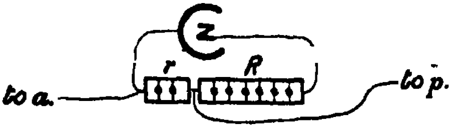
The ambiguity may be provisionally neutralised by making use of the expression "optimal minimal" for the true minimal stimulus *qua* its energy value, and by avoiding use of the term minimal for stimulation by induction shocks, or by condenser discharges of a gradient above or below the optimum or characteristic gradient.

The *method* followed will be most readily understood by consideration of the diagram.

(A) The apparatus for excitation, composed of battery, condenser, and a Morse key, is connected with the nerve of a nerve-muscle preparation by unpolarisable electrodes, the circuit being arranged so that charge of the condenser is at contact *a*, and does not traverse the nerve, while discharge is at contact *b*, and alone traverses the nerve During excitation the key K_1 is closed, and the key K_2 open.

(B) The apparatus for measurement of resistance, composed of battery, Wheatstone bridge, and galvanometer, is connected with the nerve by opening the key K_1 (and closing the key K_2)

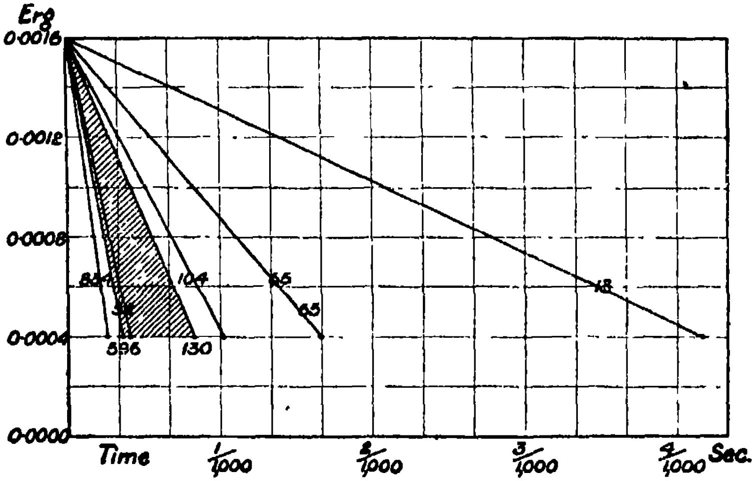




To obtain fractions of a volt the disposition figured in the smaller diagram † was adopted.

The muscular contraction was watched for, and occasionally recorded in the usual way.

As regards the human subject, the circuit was as before, a Stohrer's battery up to 60 volts being used to charge the condenser One large electrode (the "indifferent" electrode) was strapped to the abdomen, a second small electrode (the "testing" electrode) was fixed over an accessible nerve, such as the ulnar, median, or peroneal.



To illustrate rate of energy discharge and approximate gradients when the constant has not less than two digits nor more than three digits The shaded area indicates range within which the nerve characteristic must have fallen

The procedure was of three types —(1) To take a series of combinations between voltage and capacity, so as to deliver constant energy at various rates, (2) to take a series of voltages and find for each voltage the smallest capacity at which contraction was visible; (3) to take a series of capacities and find for each capacity the smallest voltage at which contraction was visible.

The first plan has the considerable advantage of permitting the use of a myograph. It is chiefly serviceable for the purpose of a preliminary orientation, *i e*, when it is desired to find quickly the range at which the characteristic is to be looked for. Having obtained a minimal effect with any given voltage and capacity, the next test is to be made with half the voltage and four times the capacity, *i e.*, with the same energy as before, but with that energy falling by a curve with a constant equal to one-eighth of the constant of the previous case. If the effect is manifestly greater, the voltage is again to be halved and the capacity quadrupled from the starting point of a new minimal effect. If, on the other hand, after the first step in reduction of the constant, there is no effect at all, a test is taken in the opposite direction, with double the voltage and one-fourth the capacity, *i.e.*, with the constant of the original energy curve multiplied by eight.

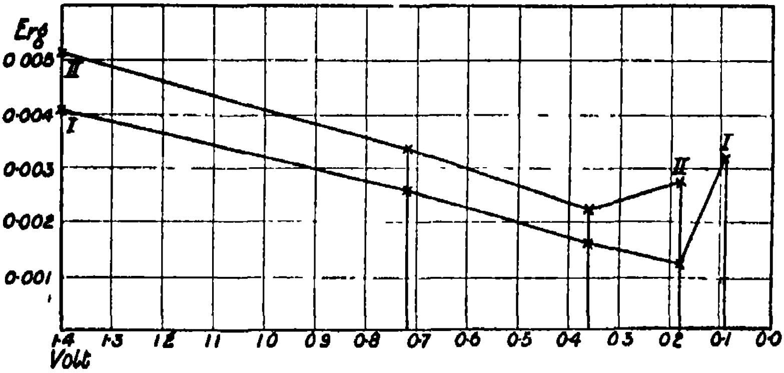
The second plan affords a rapid survey of the whole field of stimulation. When energy is varied by variation of voltage at any given capacity, it varies as voltage squared, and the constant of its curve is augmented directly as the voltage. The step from maximal to subminimal stimulation is comparatively large when we test at a series of diminishing voltages, and a voltage is soon reached at which no increase of capacity, however great, can bring out an effective stimulus, the constant of the energy curve being lowered as capacity is increased.

The third plan is useful only for closer determination of an optimum gradient, when the range within which it is to be sought for is approximately known. When energy is varied by variation of capacity at any given voltage, it varies directly as capacity, but the constant of its curve diminishes as capacity is increased, *i.e.*, a small increment of energy is obtained at a reduced gradient, as compared with a large increment of energy at a raised gradient obtained by increase of voltage.

Exp. 1.—Nerve-muscle Preparation. Resistance of Nerve + Electrodes = 150000 ω

I	Capacity in micro-farads	Pressure in volts	Quantity in micro-coulombs	Energy in ergs.	Constants	Time of fall of energy to ½ value	Contraction.
	F.	V	FV.	5FV²	C.	$t_{\frac{1}{2}}$	
	0·0004	1·44	0·000576	0·00415	20850	0·000012	small.
	0·0016	0·72	0·001152		2606	0·000168	large.
	0·0010	0·72	0·00072	0·00259	4170	0·000104	small
	0·0040	0·36	0·00144		521	0·000416	large
	0·0025	0·36	0·0009	0·00162	834	0·000260	small
	0·0100	0·18	0·0018		104	0·001040	large.
Opt	0·0080	0·18	0·00144	0·00130	130	0·000832	small
	0·0320	0·09	0·00288		16	0·003328	none.
	0·0800	0·09	0·0072	0·00324	6·5	0·008320	small
	0·3200	0·045	0·0144		0·8	0·033280	none.
Fig	0·0400	0·09	0·0036	0·00150	834	0·000260	none.
	0·0100	0·18	0·0018		104	0·001040	large.
	0·0025	0·36	0·0009		13	0·004160	small

Exp. 1.



The constant of the optimal minimal stimulus (*i.e.*, the characteristic) is 130. The energy falls to ¼ of its original value in nearly $\frac{1}{1000}$ sec.

The second portion of this experiment, although less typical than the first, exhibits a similar character. The minimal stimulus is higher and of higher gradient, but further experiments will be required before we may admit this latter change to be other than an accidental effect. In other experiments there has been diminution of minimal stimulus with lower gradient.

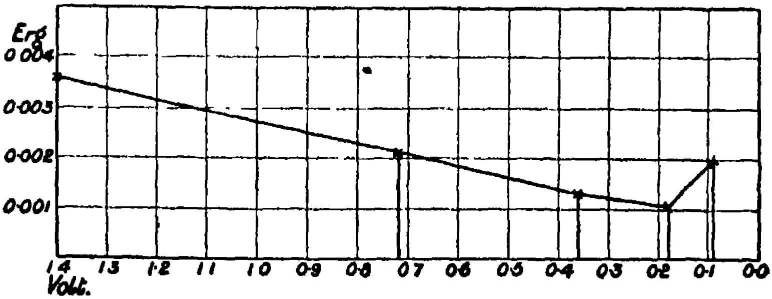
Exp. 1 (repeated.)

II	F	V	FV	5FV²	C	t_1	Contraction
	0·0005 0·0020	1·44 0·72	0·000720 0·001440	0·00518	16680 2085	0·000052 0·000208	small large.
	0·0013 0·0052	0·72 0·36	0·000936 0·001872	0·00337	3210 401	0·000135 0·000540	small large.
Opt	0·0035 0·0140	0·36 0·18	0·001260 0·002520	0·00227	596 74	0·000364 0·001456	small none.
	0·0170 0·0680	0·18 0·09	0·003060 0·006120	0·00275	61 8	0·001770 0·007080	small. none.
	0·0160 0·0040 0·0010	0·18 0·36 0·72	0·002880 0·001440 0·000720	0·00259	4170 521 65	0·000104 0·000416 0·001664	small large none

Exp. 2.—Frog. Sciatic Nerve. R = 80,000 ω.*

	Capacity in micro-farads F	Pressure in volts. V	Quantity in micro-coulombs FV.	Energy in ergs. $5FV^2$.	Constant. C.	Time of fall of energy to ½ of its original value t
						sec.
	0·00035	1·44	0·000504	0·003629	44700	0·000019
	0·00085	0·72	0·000612	0·002203	9200	0·000047
	0·00200	0·36	0·000720	0·001296	1950	0·000111
Opt	0·00700	0·18	0·001260	0·001134	**279**	0·000388
	0·05000	0·09	0·004500	0·002025	19	0·002770
No effect.	10·00000	0·015	0·450000	0·101250	0·05	0·554000
	0·0001	5·00	0·0005	0·012500	543000	0·000005
	0·0010	1·00	0·0010	0·005000	10850	0·000055
Opt	0·0100	0·20	0·0020	0·002000	**217**	0·000554
	0·1000	0·13	0·0130	0·007450	14	0·005540
	1·0000	0·10	0·1000	0·050000	1	0·055400

Exp 2.



Influence of Temperature.—The characteristic of nerve is very sensitive to alterations of temperature, being raised by high temperature (30°), and depressed by low temperature (5°). Gotch and Macdonald have shown that stimulation of nerve by break induction shocks is favoured by heat, disparaged by cold, and that stimulation by the constant current is favoured by cold, disparaged by heat (13) † The present experiments show clearly that short stimuli (energy curve of high number) are

* In the first group of trials given voltages are taken, and the minimum effective capacities sought for The characteristic = 279.

In the second group given capacities are taken, and the minimum effective voltages sought for The characteristic = 217.

† G. and M. allude to condenser excitation, but did not employ it correctly They supposed that a "short" minimal stimulus could be obtained from a condenser of 0·5 microfarad.

effective at high temperature, ineffective at low temperature, while long stimuli (energy curve of low number) are effective at low temperature, ineffective at high temperature.

These results indicate further a point of probably considerable biological significance, inasmuch as the characteristic of frog's nerve at high temperature is found to approximate to that of mammalian nerve. Thus, whereas the characteristic of frog's nerve at room temperature (16° to 18°) is represented by a number of three digits, and that of human nerve at normal temperature (37°) by a number of five digits, the approximate characteristic (i e, the constant indicating the gradient of a suitable stimulus) of frog's nerve at high temperature (30°) is also expressed by a number of five digits These and other data are tabulated in the concluding summary.

Exp 3—R = 70,000 ω *

T.	F.	V.	FV	5FV²	C	$t_{\frac{1}{4}}$	
						sec	
30°	0·0015	1·44	0·0022	0·015	11910	0·000073	Abolished by cooling
5	0·5000	0·24	0·1200	0·139	6	0·021250	Abolished by warming.

For the first trial at 32° the voltage is taken at 1·44, and the minimum effective capacity is sought for and found to be a little below 0·0015

In the second trial at 5°, the capacity is taken at 0·5, and the minimum effective voltage is sought for and found to be a little below 0·24.

The first or short stimulus is immediately rendered ineffective by cooling.

The second or long stimulus is immediately rendered ineffective by warming.

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* In experiments 3 and 4 the characteristic proper has not been determined, but only the constants of a short and long curve of minimal but not optimal minimal stimuli. An optimal minimal stimulus is rendered ineffective by heating and by cooling. The alterations of resistance by alterations of temperature have not been taken into calculation. Such alterations would, however, have only intensified the contrast already apparent between short and long stimuli, in accordance with the following numbers -

T.	F.	V.	FV.	5FV²	C.	$t_{\frac{1}{4}}$.	R.
						sec	
30°	0·0015	1·44	0·0022	0·015	16700	0·000052	50,000 ω
5	0·5000	0·24	0·1200	0·139	4·17	0·034600	100,000 ω
30°	0·0007	1·44	0·001	0·007	17900	0·000048	100,000 ω
5	1·0000	0·09	0·090	0·040	0·391	0·139000	200,000 ω

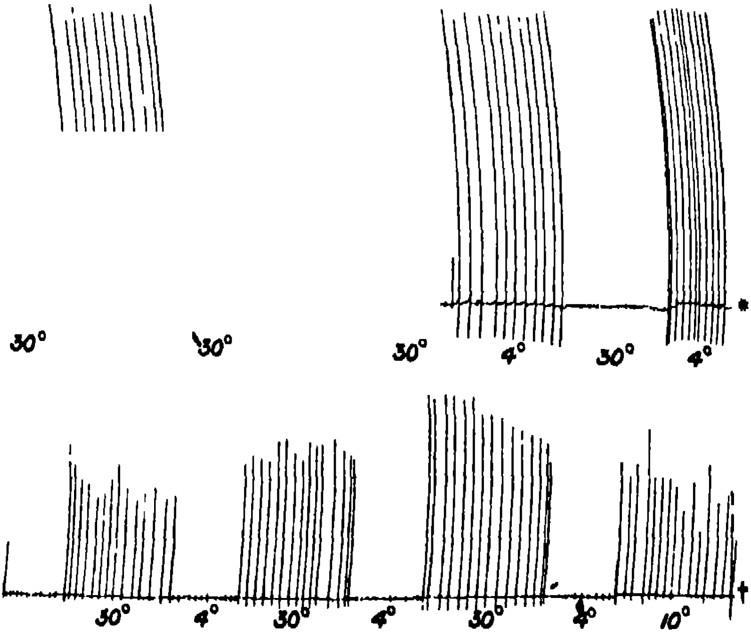
Exp 4.—Effects of High and Low Temperature upon Characteristic of Nerve.

$R = 130000\ \omega$

T.	F	V	FV	5FV²	C	$t_{\frac{1}{4}}$.	
						sec	
30°	0·0007	1·44	0·001	0·007	11382	0·000063	Abolished by cooling
4°	1·0000	0·09	0·090	0·040	0·6	0·000100	Abolished by warming

Procedure similar to that of previous experiment.

The characteristic is raised by heat, lowered by cold, as in the previous experiment.



* 1 microfarad, 0·09 volt. † 0·0007 microfarad, 1·44 volt.

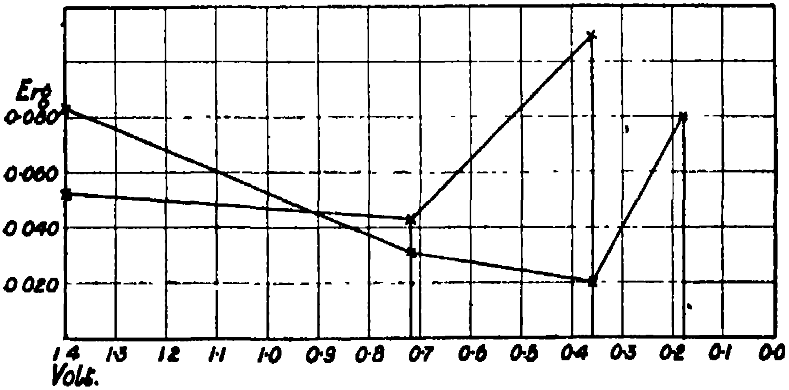
Exp. 5.—Cat. Sciatic Nerve. R = 17000 ω

	F.	V.	FV	5FV^2		t_1 sec
	0·0022	2·88	0·0063	0·071	66910	0·000026
	0·0050	1·44	0·0072	0·052	11700	0·000059
Opt	0·0170	0·72	0·0122	0·044	**2165**	0·000202
	0·1700	0·36	0·0612	0·110	108	0·002023
No effect {	0·5000	0·18	0·0900	0·081	18	0·005890
	1·0000	0·18	0·1800	0·162	9	0·011780

About 20 minutes later

	0·006	2·88	0·0173	0·249	21530	0·000071
	0·008	1·44	0·0115	0·083	9200	0·000094
	0·012	0·72	0·0086	0·031	3066	0·000141
Opt	0·030	0·36	0·0108	0·019	**613**	0·000353
	0·500	0·18	0·0900	0·081	18	0·005890

Exp. 5



The method of trial was to start with given voltage and find minimum effective capacities.

In the first group of trials the characteristic is 2165; in the second it is 613.

The constants of minimum effective stimuli at various voltages are higher in the first than in the second group.

Short stimuli are relatively more effective in the first group, long stimuli in the second group

Exp. 6.—Cat. Phrenic Nerve. R = 30000 ω.

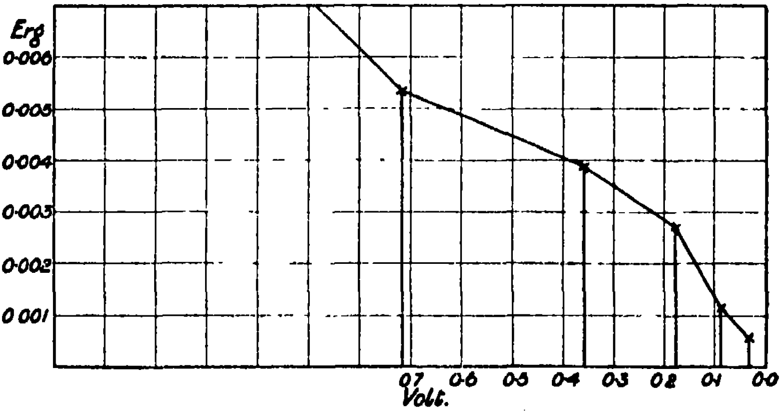
F.		FV.	5 FV²	C.	
					sec
0·0012	1·44	0·00173	0·012442	34700	0·000025
0·0021	0·72	0·00151	0·005443	9960	0·000044
0·0060	0·36	0·00216	0·003888	1740	0·000125
0·0170	0·18	0·00306	0·002754	307	0·000353
0·0300	0·09	0·00270	0·001215	87	0·000624
0·1000	0·036	0·00360	0·000648	10·4	0·002080

A few minutes later

0·0085	1·44	0·01224	0·088128	4910	0·000177
0·6000	0·36	0·21600	0·388800	174	0·012500

Voltage given, capacity looked for

Exp. 6.



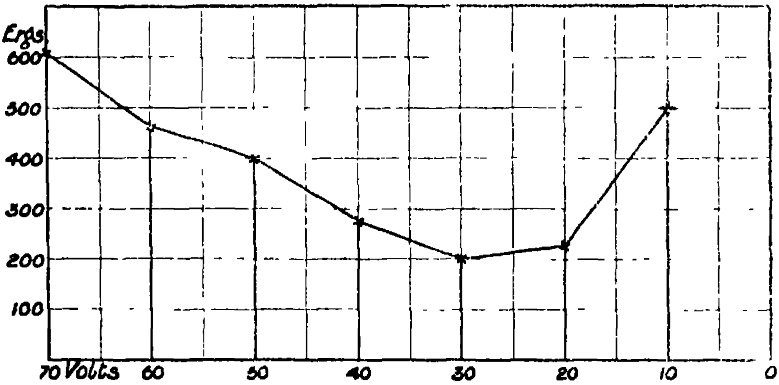
Exp. 7.—Man A Ulnar Nerve R = 12000.

	F.	V	FV	5FV²	C.	$t_{\frac{1}{2}}$
	mf	volts	mc.	ergs		sec.
	0·8000	10	8·00	400	905	0·006650
	0·1400	20	2·80	280	10350	0·001165
t .. .	0·0550	30	1·65	247·5	39500	0·000458
	0·0350	40	1·40	280	82500	0·000291
	0·0250	50	1·25	312·5	145000	0·000208
	0·0180	60	1·08	324	241500	0·000150
	0·0150	70	1·05	367·5	337500	0·000125

Exp. 8 —Man B. Ulnar Nerve.

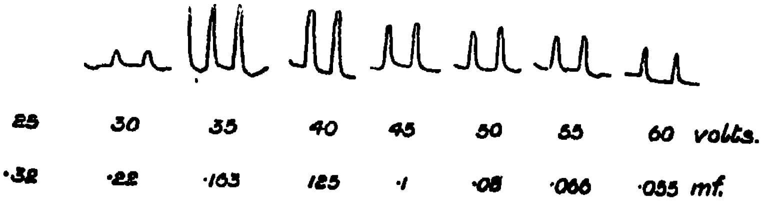
	F.	V.	FV	5FV²	C	t_1
	mf	volts.	mc	ergs.		sec
	1 0000	10	10 ·00	500	725	0 008320
	0 1150	20	2 30	230	12600	0 000956
Opt.......	0 0450	30	1 35	202·5	48250	0 000374
	0 0350	40	1 40	280	82500	0 000291
	0 ·0320	50	1 60	400	113000	0 ·000266
	0 0260	60	1 56	468	167000	0 000216
	0 0255	70	1 78	615	200000	0 000212

Exp. 8.



Exp. 9 —Man. Peroneal Nerve R = 15,000 ω

	F	V	FV	5FV²	C	t_1	H
	mf	volts	mc	ergs		sec	mm
	0 ·5000	20	10 00	1,000	2316	0 ·005200	0
	0 3200	25	8 00	,,	4525	0 003330	trace
	0 ·2200	30	6 66	,,	7825	0 002310	1 5
Opt	0 ·1633	35	5·72	,,	12400	0 001700	6·5
	0 ·1250	40	5 00	,,	18530	0 001300	6 0
	0 ·0988	45	4 15	,,	26375	0 ·0010300	4 5
	0 ·0800	50	4 ·00	,,	36200	0 000832	4 0
	0 0661	55	3 ·64	,,	48200	0 ·000687	3 ·0
	0 0556	60	3 34	,,	62500	0 000578	2 5



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10 Cybulsky and Zanietowsky "Ueber die Anwendung des Condensators zur Reizung der Nerven, &c," 'Pfluger's Archiv,' vol 56, 1894, p 45

11 Hoorweg "Ueber die elektrische Nervenerregung," 'Pfluger's Archiv,' vol 52, 1892, p 87

12 Hoorweg "Ueber die Nervenerregung durch Condensatorentladungen" 'Pfluger's Archiv,' vol 57, 1894, p 427

13 Gotch and Macdonald "Temperature and Excitability," 'Journal of Physiology,' vol 20, 1896, p 285

Addendum received February 10, 1899

Sensificatory Stimuli.

In the attempt to determine a "characteristic" of various modes of sensificatory stimulation, I encountered the doubt, and, therefore, difficulty, inherent to all investigations where the experimental criterion is a subjective one—by appreciation of minimal sensation. This broad distinction was, however, clearly apparent, that whereas for series of stimuli of motor nerves the minimum exciting energy decreased to a minimum from which it rose again (diminishing voltage and increasing capacity), the energy of sensory stimuli of increasing duration decreased indefinitely towards a minimum value. Within the limits to which my experiments extended, decrease of energy was made up for by increase of its duration.

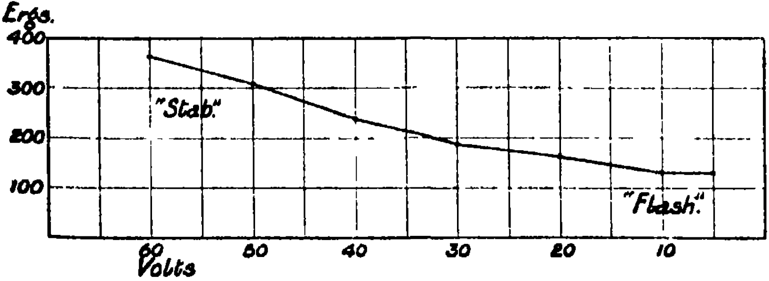
Testing on the human subject for minimal felt effect, by looking for minimum effective capacity at a series of given voltages, I found it difficult to distinguish between apparently different qualities of minimal sensation. In the case of cutaneous nerves, it was generally impossible to say whether the smallest "something felt" was directly cutaneous or directly subcutaneous, or indirectly subcutaneous, by reason of some muscular contraction In the case of the eye it was often difficult to tell whether the smallest "something felt" were a slight common sensation or a slight phosphene

In the case of the eye this point, however, came out with satisfactory distinctness, viz, that low in the energy series, *i e*, with longer stimuli by lower voltage at greater capacity, the subject of experiment is more conscious of a "flash" (optical sensation) than of a "stab" (common sensation), whereas high in the energy series, viz., with shorter stimuli by higher voltage at lower capacity, the subject feels a "stab" (common sensation) more readily than a "flash" (optical sensation). This indicates that longer stimuli are better adjusted to the excitability of the retina, shorter stimuli to the excitability of common sensory terminal organs.

Eyeball. R = 12000 ohms

	F	V	FV	$5FV^2$	C	t
	0·020	60	1·2	360	217000	0·000166
Pain.	0·025	50	1·25	312·5	145000	0·000208
	0·030	40	1·2	240	96500	0·000250
	0·040	30	1·2	180	54300	0·000333
	0·080	20	1·6	160	18100	0·000665
Flash ...	0·250	10	2·5	125	2900	0·002030
	1	5	5·0	125	362	0·008320
	0·015	60	0·9	270	290000	0·000125
Pain .. .	0·020	50	1·0	250	181000	0·000166
	0·030	40	1·2	240	96500	0·000250
	0·040	30	1·2	180	54300	0·000333
	0·060	20	1·2	120	24100	0·000499
Flash.....	0·200	10	2·0	100	3620	0·001660
	0·500	5	2·5	62·5	724	0·004160

Pain to short stimuli, light to long stimuli.



This principal conclusion is confirmed by experiments conducted on the first plan of procedure, viz., by testing with stimuli of equal energy at various gradients. To this end I found it convenient to first settle upon a minimal sensation at a given mean voltage, and then to make two further trials, one at half the voltage and four times the capacity, the other at double the voltage and one-fourth the capacity. Each group of three tests was thus at the same energy, but with the constants of the three discharge curves in the proportion 1, 8, 64. The minimal sensation having been settled with the constant at 8, it could be inferred from equality or inequality of the effects at higher and lower constants, whether the constant of a fitting stimulus lay above or below either of the two extremes.

	F	V.	FV.	5FV².	C	t_1	Sensation.
				ergs			
Forearm, common sensation R = 45000	0·4000 0·1000 0·0250	12 24 48	4·8 2·4 1·2	288 288 288	579 4632 34056	0·001250 0·000313 0·000078	None. Slight. Marked.
Tongue, common sensation R = 7500	0·0800 0·0200 0·0050	12 24 48	0·96 0·48 0·24	57·6 57·6 57·6	17380 139040 1112320	0·000416 0·000104 0·000028	Marked Slight. None.
Optical sensation R = 21000	0·2100 0·0600 0·0150	12 24 48	2·88 1·44 0·72	172·8 172·8 172·8	2031 16248 129984	0·003493 0·000873 0·000218	Marked Slight. None
Movement, median nerve R = 30000	0·2000 0·0500 0·0125	12 24 48	2·4 1·2 0·6	144 144 144	1706 13648 109184	0·004158 0·001040 0·000280	(Movement). None. Slight. None

To verify the statement that at equal energy retinal stimulation predominates when the gradient is low, and common sensory stimulation when the gradient is high, it is preferable to take stimuli above the minimal and with larger difference of gradient, as *e g.*, in the following experiment.—

Anode on Eyeball. Kathode on Abdomen. R = 9000 ohms.

F.	V.	FV.	5FV².	C	t_1	Sensation.
mf.	volts.	mc	ergs		sec	
0·04	54	2·16	583·2	130200	0·000250	Stab only.
0·36	18	6·48	583·2	4828	0·002245	Stab + flash.
3·24	6	19·44	583·2	179	0·020200	Flash only.

"The Parent-rock of the Diamond in South Africa." By Professor T. G. BONNEY, D.Sc., LL.D., V.P.R.S. Received May 1,—Read June 1, 1899

So much has been written on the occurrence of diamonds in South Africa that a very few words may suffice as preface to this communication. References to many papers on the subject are given in 'The Genesis and Matrix of the Diamond' (1897), by the late Professor H. Carvill Lewis,* and others have been published since that date.† It may suffice to say that the diamond, first discovered in 1867 in gravels on the Orange River, was found three years later in certain peculiar deposits, which occur locally in a region where the dominant rock is a dark shale, sometimes interbedded with hard grits, or associated with igneous rocks allied to basalt. These deposits occupy areas irregularly circular in outline, and bearing a general resemblance to volcanic necks. The diamantiferous material, near the surface, is soft, yellowish in colour, and obviously much decomposed; at a greater depth it assumes a dull greenish to bluish tint, and becomes harder. At the well-known De Beers Mine, near Kimberley, the works in 1898 had been carried to a depth of about 1,500 feet, and the diamantiferous material, for at least the last 100 yards, was not less hard than an ordinary limestone. It has a brecciated aspect, the dark, very minutely granular, matrix being composed mainly of serpentine (about four-fifths of the whole), and of a carbonate of lime (with some magnesia and a little iron). In this matrix are embedded grains of the following minerals:—Olivine, enstatite, smaragdite, chrome-diopside (omphacite of some authors), a brown mica, garnet (mostly pyrope, but more than one variety observed), magnetite, chromite, ilmenite, with several other minerals much more sparsely distributed.

Rock fragments are also present, variable in size, but commonly not exceeding about an inch in diameter, as well as in quantity. These, occasionally, but not generally, are rather abundant. In some cases they are chips of the neighbouring black shale, but in others they are greyish-coloured with somewhat of a porcelain aspect. The latter are generally sub-angular in form and externally banded or bordered with a darker tint; crystalline rocks have also been noticed, though these appear to be far from common, such as granite, diorite, and varieties of eclogite.[*] As to the genesis of the diamond, more than one opinion has been expressed. Professor Lewis regarded the matrix as a porphyritic form of peridotite, once a lava, now serpentinised,[†] in which the diamond had been formed by the action of the molten rock on some carbonaceous material (probably the Karoo shale). Others regarded the matrix as a true breccia, comparing it with the agglomerates in volcanic rocks. But among the latter, some thought that the diamond had been produced *in situ* by the action of steam or hot water in a subsequent solfataric stage of the volcano, while others (including myself) held that it had been formed, like the garnets, pyroxenes, &c., in some deep-seated holocrystalline mass which had been shattered by explosions.[‡]

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* Edited by the present writer.

† Jules Garnier, 'Geol. Soc. South Africa Trans.,' 1897, p. 91; H. S. Harger, *ibid.*, p. 124. See also W. G. Atherstone, *ibid.*, 1896, p. 76; L. De Launay, 'Compt. Rend.,' vol. 125 (1897), p. 335. The last author, in 'Les Diamants du Cap' (Paris, 1897), gives a very full account of the mines, but an even better one will be found in Max Bauer, 'Edelsteinkunde' (Leipzig, 1896, p. 208).

The specimens which I am about to describe were obtained at the Newlands Mines, West Griqualand, from 40 to 42 miles from Kimberley, almost due N.W. Here the workmen occasionally came across well-rounded boulder-like masses of rather coarsely crystalline rock, studded with garnets, which are sometimes about a foot in diameter. Specimens of these were found or obtained by Mr. G. Trubenbach, the London manager of the Newlands Diamond Mine Company, during a visit to the mines in 1897. His interest had already been aroused by picking up a specimen, presently to be noticed, in which some small diamonds occurred, very closely associated with a garnet, so the boulders were brought back by him to England. On careful examination a small diamond was detected on the surface of one of these. On breaking the boulder others were revealed. The most interesting fragment was sent by Mr. Trubenbach to Sir W. Crookes, who showed it to me. Examination with a hand lens convinced me that the rock could not be a concretion of the "blue ground," but was truly holocrystalline and allied to the eclogites. Sir W. Crookes generously waived his own claim to study the specimen, and obtained for me permission from Mr. Trubenbach to have slices cut from it. I gladly take this opportunity of expressing my gratitude to both gentlemen, to Sir W. Crookes, for allowing me to carry out this interesting investigation, and to Mr. Trubenbach for his great liberality in placing at my disposal a considerable suite of specimens (including other boulders) from the Newlands mines, and for the trouble which he has taken in affording me the necessary information.

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[*] A. W. Stelzner, 'Sitzungsber. u. Abhandl. der Isis' (Dresden), 1893 (April), p. 71, calls attention to the fact that these show signs of attrition and that they range in size from a few cubic millimetres upwards, being sometimes large boulders. Among the materials (at Kimberley) he mentions both granite and eclogite.

[†] For the rock itself he proposed the name "kimberlite."

[‡] In other words, that the volcano (as occasionally has happened) had ejected little or no lava or scoria, discharging only steam and hot water, with shattered rock. This view is held by Max Bauer, in 'Edelsteinkunde,' p. 225, which, however, I had not seen when this paper was written.

Prior to the discovery, just mentioned, one or two instances had occurred at the De Beers Mine of a diamond apparently enclosed by or projecting into a pyrope. One such, the garnet being the size of a rather large pea, is in the collection at Freiberg (Saxony), to which it was presented in 1892 *

The specimen found by Mr. Trubenbach at the Newlands mine, was a piece of blue ground, with a pyrope projecting from one angle A small, apparently broken, diamond seems embedded at the top The others (five) are well crystallised, two on one side, three almost in contact on the other The pyrope (which has a kelyphite rim) seems to be indented by two, but to have once included the others, as they are in contact with the unaltered mineral. We were thus brought so far as to associate the diamond with the pyrope, though this proved no more than the presence of garnets in the parent rock of the diamond, and thus made the eclogite (already known to occur) highly probable, for, as observed by Professor R Beck†, the specimen itself is blue ground. In confirmation of his statement I pulverised a fragment,‡ and find that the powder corresponds with the matrix of the blue ground when similarly treated The latest discoveries enable me to complete the chain of evidence

Eclogite Boulders containing Diamonds

The first named, that containing several diamonds, is a fragment (perhaps from a quarter to a third) of a boulder, which probably was ellipsoidal in shape, two of the axes being nearly equal and the third distinctly the longest. We may infer that it was rounded from a roughly rectangular block, since the curved surfaces are slightly flatter in the middle parts The axial lengths in the fragment (prior to removing a piece from one end) were approximately 4 in by 3 in. by 2 in. The rock is coarsely granular, apparently composed of two green-coloured minerals, one darker than the other (possibly only different states of a single mineral), and of rich resin-pink coloured garnets, varying in size from a hemp seed to a pea, with slightly irregular distribution. The outer surface of the boulder, except for a very small "step" on one side, is smooth, the garnets barely, if at all, projecting. The latter are covered with a rather soft, dark skin, sometimes slightly thicker than the thumb nail, which often has partly fallen off. This, as can be seen on the broken surfaces, becomes less conspicuous in the inner part of the boulder, and is sometimes invisible to the unaided eye

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* A. W. Stelzner, 'Sitzungber der Isis zu Dresden,' 1893, s. 85, and R. Beck, 'Zeitsch. für praktische Geologie,' 1898 (May), p. 163.

† *Ut suprà.*

‡ I could not advise Mr. Trubenbach to have a slice cut from the specimen, as I feared it might be injured, but he kindly detached a little fragment from the opposite end to that named above, which I have thus examined.

Two small diamonds are exposed on the curved outer surface, one about half the other about one-fifth of an inch from the edge of the cross fracture. On the latter surface, nearly an inch below the last named, three small diamonds appear to lie in a line touching one another, and near them are two others,* all four within a space about three-quarters of an inch square, an eighth diamond is about an inch and a half away (on the same face), a ninth, about one-fifth of an inch from the top edge, and a tenth occurs on the larger cross-fractured surface, but near to the edge of the other one. These diamonds are octahedra in form, generally with stepped faces—one, at least, apparently twinned—perfectly colourless, with brilliant lustre, the largest being quite 0·15 inch from apex to apex, the smallest not exceeding 0·05 inch. All seem to be embedded in the green part of the rock. As the outer part of the boulder looks rather more decomposed than the inner, I had a piece removed from one end, thus enabling me to study the mass to a depth of more than an inch from the surface, and examined a strip, about 4 inches long, in a series of five slices

The late Professor Lewis has given, in the volume already mentioned, so full an account of the minerals which occur in the "blue ground," that it will be needless on the present occasion to do more than refer to his descriptions,† only calling attention to any variations in the mineral constituents and their association in these eclogites. These constituents are .—

1. (*a*) *Garnet* (Pyrope).—In the slice these appear a light tawny or yellowish-red tint, retaining this tint (though much lighter) under the microscope ‡ They are generally clear, with frequent and irregular cracks, but are occasionally traversed by wavy bands of minute enclosures of a pale brown filmy mineral, which is rather irregular in outline, very feebly pleochroic, and gives with crossed nicols fairly bright polarisation tints Similar minerals sometimes have formed along the cracks They are probably mica, or possibly chlorite, and indicate incipient decomposition. The garnets towards the outside of the boulder, as already said, are enveloped in a "skin," and the microscope shows that it usually exists inside, though there it is thinner. In the former case it is generally browner in colour and more distinctly crystalline, corresponding in cleavage, pleochroism, &c , with a mica of the biotite group, in the latter it is greener and more filmy with an aggregate habit and seems to project into the garnet. I regard it as due

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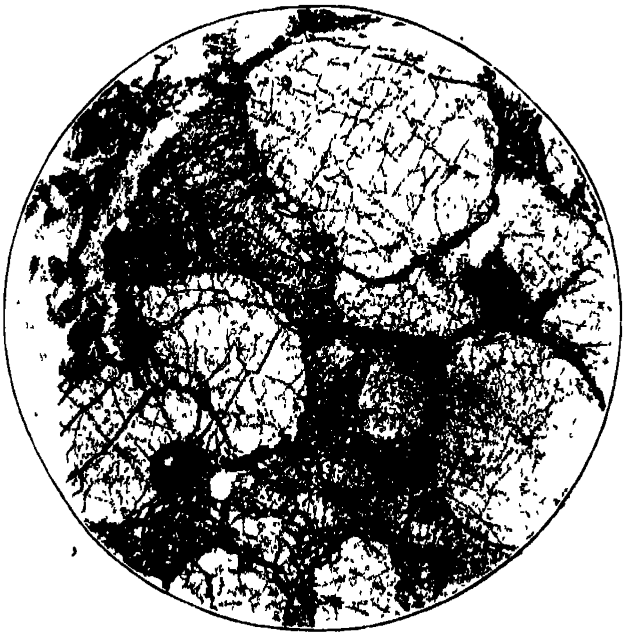
* It is possible that these two form a twin crystal, but I think they are separate. As the point is unimportant, I have not attempted to clear away the matrix.

† We must also not forget the paper by Professor Maskelyne and Dr. Flight ('Quart Journ. Geol Soc ,' vol 30, p. 406), in which several of these minerals are described, analysed, and identified. In fact the authors ascertained everything that was possible with the materials then obtainable.

‡ Unless it is expressly stated, the use of a 1-inch objective may be assumed.

FIG. 1.—Section of the diamond-bearing eclogite. Pyropes with narrow kelyphite borders, and chrome diopside intercrystallised. The clinopinacoidal cleavage of latter visible in the lower part of the section.



to decomposition, a form of the well known kelyphite rim, sometimes a mica, sometimes a chlorite, possibly now and then associated with a little minute hornblende. In a few cases a "rim" is brown in the outer part and green within. The constituents tend to a parallel her than a radial grouping. The garnets occasionally contain ite branching root-like enclosures grouped in bands. Though these n polarised light, I regard them as empty cavities, and attribute this to diffraction.

(*b*) *Chrome-diopside.*—The mineral described under that name by Professor Lewis, and referred to by others as omphacite or sahlite. The individuals are sometimes about a quarter of an inch long. In thin slices it is a pale dullish green colour, inclining to olive; under the microscope, a pale sea-green, with a trace of pleochroism. It has one strongly marked cleavage, not however nearly so close as in ordinary diallage, and a second weaker, sometimes approximately at right angles to it.* On examining flakes, obtained by crushing, I find the strong cleavage to be clinopinacoidal and the other probably basal, and obtain on a clinopinacoid an extinction of 35° with a prism edge. It is in fact identical with the pyroxene described by Professor Lewis* as chrome-diopside. In it (though rare) are small rounded enclosures of a greenish mineral aggregate much blackened with opacite I regard them as alteration products of a ferriferous olivine. This diopside, at the exterior and along cracks, is often converted into a minutely granular to fibrous mineral, which gives a "dusty" aspect to that part of the crystal, when viewed with transmitted light, and a whitish-green one with reflected light This often terminates in a minutely acicular fringe, piercing the original diopside. Its grains occasionally are a little larger, showing a cleavage, dull green in colour, fairly pleochroic, and having the extinction of hornblende. A process of secondary change, as in uralite, is no doubt indicated. Now and then a tiny film of brown mica occurs in this part or even in a crack in the diopside.

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* One may give a general idea of their relative importance by comparing them to the columns and cross-joints in some basalts.

It is this alteration product which gives the mottled aspect mentioned above as visible to the unaided eye, so this is not indicative of a third important constituent in the original rock. In one of the slices the mica just named attains a larger size (about 0·03 inch across), has a fairly idiomorphic (hexagonal prism) outline, and is not restricted to the margin of the garnet In this case it is generally associated with calcite,† which it tends to surround, and that in one place encloses a radiating acicular mineral (? a zeolite); in another the calcite, or some other carbonate, is mixed with a serpentinous material. Distinct granules of iron oxide are practically absent from the slices, though here and there it may be indicated by some opacite. I have not found spinel, or rutile, or zircon, or pseudobrookite. In fact, putting aside the diamonds, the rock in its unaltered condition was a coarsely holocrystalline mixture of chrome-diopside and garnet, with a few small enclosures of olivine, in other words, it was a variety of eclogite and of igneous origin ‡

2. A fragment (probably about one quarter) of a flattish ovoid boulder.—The two broken surfaces, which are nearly at right angles, measure 5 and 5½ inches, roughly, and it is about 3½ inches high. The rock very closely resembles the one just described, except that mica occurs rather oftener and in larger flakes; perhaps the garnets (here also not quite regularly distributed) are slightly more numerous. The outer

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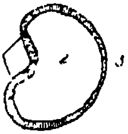
[*] *Loc. cit.*, p 21.

[†] From the facts I think it probably of secondary origin. It reminds me sometimes of the brown mica produced by contact metamorphism.

[‡] I am, of course, aware that eclogite, in the past, has been regarded by some geologists as a metamorphic rock. Apart from the fact that several rocks once assigned to this class are now, with good reason, regarded as igneous, I have had several opportunities of studying eclogite, and have no doubt as to its origin. Take away the alkali from a magma with the chemical composition of a diorite, and the result would be garnets in place of felspar, *i.e.*, an eclogite.

surface is not quite so well preserved, though enough remains to show that it also has been smooth, and a few thin veins of a white mineral (calcite ?) traverse the rock. On this surface, near the meeting of the two fractures, and exposed by the removal of a little material (*i.e.* it might originally have been just hidden) is a diamond (octahedron), apparently about 0·1 inch in diameter. On one side it rests against a pyrope, the adjacent surface of which is incurved, the two minerals being parted by the dull green-coloured kelyphite rim of the latter, which is about 0·03 inch in thickness. Thin sections of this boulder correspond almost exactly with those from the other, the garnets showing precisely the same tints, though traces of a cleavage, (roughly parallel throughout) are perceptible on close inspection, and are distinct under the microscope. In garnet such a structure commonly indicates pressure, and the general parallelism accords with this explanation, but the other constituents show no signs of crushing. The "kelyphite" rims to the garnets are perhaps slightly broader and the brown mica passes into a green (chloritic ?) mineral, and occupies cracks in the garnet a little more frequently, but as before the constituents tend to lie parallel rather than radially. One or two of the diopsides show fine oscillatory twinning. The cracks are occupied with calcite or some allied carbonate. There is no real difference between this eclogite and the last-named one.

FIG. 2.—Garnet and diamond (diagrammatic, nearly twice natural size). (1) Diamond, (2) garnet, (3) kelyphite rim.



Eclogite Boulders without Diamonds.

3. Part of a boulder, which must have been about a foot in diameter (found at 250 ft. level). It presents a general resemblance to the rocks described above, with, however, the possibility of a second green constituent. This is not confirmed on microscopic examination. The rock consists, practically, of pyrope and diopside, as already described, except that negative crystals are rather unusually conspicuous in the latter. Into the details of these, as the point seems not to have any bearing on the present investigation, I do not purpose to enter.

4. A fragment, more irregular in form than the others, measures very roughly, about 7 in. by 4¾ in. by 3½ in. It retains a good piece of the outer surface, which, though now a little corroded, was once

smooth. The rock, which is rather decomposed and crumbly, consists chiefly of three minerals: garnet, not quite so large, paler and more pink in colour than the last-named; an emerald-green pyroxene, and a yellowish or greenish-grey, platy to fibrous mineral, suggestive of a second more altered pyroxene. In thin slices the paler and pinker tint of the garnet is very perceptible, as well as the tendency to a rude and generally parallel cleavage. But we find in it, under the microscope, a few microlithic enclosures, of an apparently colourless mineral, which occurs in long prisms crossed at about 70° by an occasional transverse cleavage, and extinguishing at an angle of about 26° with the longer edge. Many of the cracks exhibit slight decomposition, starting from them, and are sometimes occupied by calcite. The pyroxene, under the microscope, hardly differs from the one already described, except that the green tint is slightly richer and one or two crystals contain the small dark brown negative crystals, common in hypersthene and diallage. The dominant cleavage, as before, is along the clinopinacoid.* The third mineral proves to be an altered enstatite, but I leave the details for the present, as it is better preserved in another rock. A fourth constituent is also present, but more sparingly, viz., a pale brown mica, only moderately pleochroic (phlogophite ?). It occurs generally in plates, averaging about 0.1 inch long. The minerals appear to have formed in the following order: (*a*) garnet, (*b*) diopside, (*c*) mica, (*d*) enstatite. As before, iron oxides are very inconspicuous; there may be a grain or two (small) of serpentinised olivine. The marked presence of enstatite distinguishes this rock from the others, but it differs from the eulysites by the substitution of that mineral for olivine, and so links those rocks to the more ordinary eclogites. The occurrence of a little mica indicates the presence of a small amount of an alkali in the magma. If necessary we may name it newlandite, but personally I should prefer to call it an enstatite-eclogite, for I think the coinage of fresh titles more often a bane than a boon to science.

5. This boulder is almost perfect, except that the general flatness of one side indicates either traces of an old fracture or considerable loss by crumbling. The surface has been smooth, but it has suffered from unequal weathering of the minerals. Its girth, in three directions at right angles, is approximately 20½ in. by 19½ in. by 17½ in. It appears only to differ from the last-described in having its garnets a shade more purple, and in an approach to a banded structure; the diopside being rather more abundant in a middle zone, the garnet in one, the enstatite in the other of the outer zones. Being satisfied that it is merely a

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* As noticed by Professor Lewis, *ut supra*, p. 22, in the diopside the prism cleavage has practically disappeared, and a clinopinacoidal cleavage replaces the orthopinacoidal usual in diallage.

variety of the last-described rock, I have preferred to leave it as an intact boulder.

6. The next fragment, measuring about 3 in. by 2¼ in. by 2 in., and retaining part of its smooth outer surface, is labelled "found in the yellow ground of No. 2 mine,* 50 feet deep." Though it is much more decomposed than the others, the purplish garnet, the emerald-green pyroxene, the altered enstatite (here very rotten), and a flake or two of phlogophite (?) are easily made out. It is obviously a more decomposed specimen of the rock represented by the two preceding specimens.

7. The last of this group of specimens is a rock fragment,† measuring about 3½ in. by 2 in. in length and breadth, and slightly exceeding an inch in greatest thickness. Its outline is irregular, being determined by the fracture of the predominant diallage-like mineral. The crystals of this run large, an inch or more in length, breadth, and thickness. It is greyish-green in colour, having one dominant cleavage, with a sub-metallic lustre, and close subordinate cleavages, giving a somewhat fibrous aspect to that surface. Between these large crystalline lumps, numerous small, ill-defined garnets (pyrope) seem crowded, so as to form fairly continuous partings, generally hardly 0·1 inch in thickness. As the readiness with which the rather soft pyroxenic constituent split away made it improbable that a good slice could be cut, and I was reluctant to injure the specimen, I contented myself with detaching a few flakes of this constituent for microscopic work, since the determination of its identity was sufficient for my purpose. These show the mineral to have one easy cleavage and a rather fibrous structure; they give straight extinction parallel with this. As the usual rings and brushes can be seen on the face of easy cleavage, the mineral belongs to the bastite group. The same is true of the enstatite in boulder (4), though, as it is slightly more fibrous, and not in quite so good a condition, the optical picture is less distinct. Thus we may name the rock from which the present specimen has been broken, a garnet-bearing bastitite.

8. This specimen, said to be a fragment of a boulder, is very different from the rest. It is a compact greenish-grey rock containing enclosures, which give it the aspect, at first sight, of a pebbly mudstone. Microscopic examination shows it to be a compact felspathic diabase, with vesicles, which have been filled up with calcite, chlorites, and other secondary minerals (probably zeolites), but not to have any special interest. Its relations appear to be with the rocks occurring in a conglomerate which we shall mention in a later paragraph.

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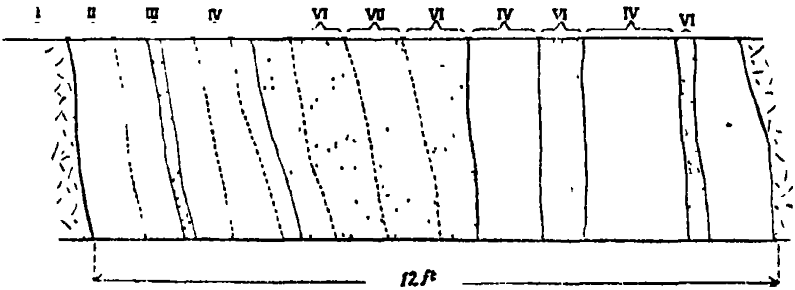
* The others come from another mine (No. 1).

† I am informed that this was not part of a boulder, but came out of the "blue ground" nearly in its present condition.

The "Blue Ground" and Associated Rocks.

Two areas of diamantiferous rock are now being worked at the Newlands Mines. The shape of the one which supplied most of the specimens described in this paper is irregular, and, so far as I know, exceptional. Its outline at the surface may be roughly compared to a rounded triangle into the base of which the point of a rather short shuttle is thrust, the greatest breadth of the two being about equal. Exploratory workings at a depth of 300 feet show that the former area rather quickly narrows, and the latter terminates in clefts. The "blue ground," in fact, appears to fill a fissure, broadening in two places to vents, which has been traced for some distance underground southwards from the principal mass of diamantiferous rock, as represented in the annexed section, where the latter is dotted.

FIG. 3



An igneous rock (i) occurs on either side. It is compact, a greenish-grey in colour, not unlike some of the less acid Welsh felstones. Under the microscope it is found to be much affected by secondary mineral changes, the iron oxides alone being in good preservation. A few small crystals of decomposed felspar are scattered in a yet more decomposed matrix, of which the minor details are uninteresting. The rock may be classed with the compact, rather felspathic, diabases. These walls of diabase, farther to the south, turn off rather sharply to east and west. In the interval, about 12 feet in width, between them, ribs of the "blue" and a mudstone alternate, the thickest one of the former being from 3 to 4 feet in width, and the inner part of it is in better preservation than the outer. Specimens have been examined from the heart of the mass (vii), a part outside it (vi), and the exterior portion (v). The first (vii) in texture, hardness, and colour reminds me a little of the dark serpentine found north of Cadgwith, in Cornwall. In this matrix roundish spots occur, some darker than it, others a yellow-green colour, besides a few angular whitish spots. The block is traversed by two or three thin calcareous veins. Specimen (vi) while generally similar is more decomposed, and apparently contains some fragments of shale Specimen (v) has a stratified aspect, being a dull grey faintly mottled rock, with streaky, dark, rather carbonaceous-looking bands, the origin being doubtful, till it is seen under the microscope. A fourth specimen (iii) shows the mudstone traversed by a vein of rather pale-coloured decomposed "blue," not exceeding an inch in thickness. A fifth (ii) is from near the diabase on the western side, a dark compact rock, faintly mottled, here and there presenting a slight resemblance to a "blue" traversed by thin veins of a carbonate, and a sixth (iv) from a like position on the opposite side is a generally similar rock, but with wider veins filled with more coarsely crystalline calcite. A seventh specimen represents the "blue" in the "neck," a few yards to the north and at the same level (300 feet) This, inferior in preservation to the first-named, includes numerous rounded fragments a little darker than the matrix, with others, angular to subangular, some also darker and some lighter than it

A brief summary of the results of microscopic examination may suffice, as these rocks do not materially differ from specimens obtained in the De Beers Mine, of which I have published a full account *

The matrix is a mixture, in slightly variable quantities, of granules of calcite or dolomite, serpentine, pyroxene, and iron oxides, in which occur flakes with fairly idiomorphic outlines of a warm-brown mica, moderately pleochroic, corresponding with that described† in one or two specimens from De Beers Mine The prisms are about 0·002 inch in diameter, and sometimes nearly as thick This mica, which, as stated in a former paper, I consider a secondary product, occurs abundantly in all the specimens, but in that from the interior (on the whole the best preserved rock) it is locally assuming a green colour, no doubt by hydration In the specimens from the thick rib, the one last named contains mineral grains and rock fragments. Except for a few flakes of the usual mica, the former are a mixture of two fibrous minerals, the larger part corresponding with actinolite, the rest, giving lower polarisation tints, may be serpentine. This fact, and structures suggestive of the former presence of a cleavage more regular than that of olivine, make it more probable that diopside was the original mineral. Though iron oxide is present in specks and rods (especially in the worse preserved specimen), this occurs either in the outer part, or as though it had been deposited along cleavage planes. In the thin rib of "blue" (iii), some of the grains are composed partly of a fibrous mineral, as above described, and partly of a clear one, which often affords rather rich polarisation tints, and presents some resemblance to quartz. Its precise nature is difficult to determine, owing to the absence of distinctive characters, but I believe it to be of second-

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* 'Geol Mag,' 1895, p 492, and 1897, p 448

† 'Geol. Mag,' 1897, pp. 450, 451.

ary origin. Rock fragments are not common in the first (interior) specimen (vii), one, however, is probably an altered shale, and another possibly a limestone. This is bordered by a pale, pyroxenic mineral piercing into the grains of calcite In the second specimen (vi) fragments are rather common; among them are those of diabase, ranging from fine to coarse, one specimen of the latter, originally, perhaps, an inch in diameter, showing an ophitic structure, felspar and augite both being rather altered, seemingly by infiltration, and one small fragment resembles a subcrystalline limestone. Specimen (v) does not materially differ, but seems to contain more carbonate than the others The dark streaking is due to grains of iron oxide or serpentine with much opacite, rock fragments are few and small Specimen (iii) from the thin vein contains a few very small rock fragments, mudstone or shale, more or less altered, possibly also a compact diabase. The "country rock" is a mudstone, consisting of small chips of quartz and felspar, variable in size, embedded in a dusty matrix, including a carbonate, which is more abundant within about a fiftieth of an inch from the junction This part is slightly stained, but I was unable to detect any signs of contact metamorphism. Specimens (ii) and (iv) are generally similar, but the former contains some small rounded bits of varieties of diabase, and one may represent a crystalline limestone. The veins are filled with calcite and other secondary products, and are bordered by a very thin film of a brown micaceous mineral, like that described as often permeating the "blue." Both specimens suggest micromineralogical changes, such as might be produced by the passage of hot water.

Other specimens of the sedimentary rock in the immediate neighbourhood of the blue have been forwarded to me by Mr Trubenbach. One, from the adit on the southern side of the section mentioned above, is a grey mudstone, containing a flattish rectangular pebble, of a dark green compact rock. Two others are from No. 2 mine, or about 700 yards to the south-west. One, struck in the shaft at a depth of 200 feet, is a conglomerate, composed of well-rounded rock fragments, with some scattered grains of quartz. Each of the former is bordered by a zone of a crystalline carbonate (impure calcite), and the interstices are filled, sometimes by a clearer variety of the same, but more often by some minutely granular secondary product. Of the rock fragments, one is a subcrystalline dolomitic limestone; two, perhaps, are chalcedony; the remainder are igneous, the majority being varieties of diabase, sometimes rather decomposed; the rest trachytes, mainly andesites. Their general aspect and the not unfrequent presence of vesicles (now filled with viridite) suggest that they have been furnished by lava-flows. Another specimen, obtained in the same working at a depth of 400 feet, is a rather felspathic diabase, not unlike one of the varieties in the conglomerate. It is a good deal decomposed, is not improbably from a lava-flow, but does not call for a minute description.

Conclusion

Thus the diamond has been traced up to an igneous rock. The "blue ground" is not the birthplace either of it or of the garnets, pyroxenes, olivine, and other minerals, more or less fragmental, which it incorporates The diamond is a constituent of the eclogite, just as much as a zircon may be a constituent of a granite or a syenite Its regular form suggests not only that it was the first mineral to crystallise in the magma, but also a further possibility. Though the occurrence of diamonds in rocks with a high percentage of silica (itacolumite, granite, &c) has been asserted, the statement needs corroboration. This form of crystallised carbon hitherto has been found only in meteoric iron (Canyon Diablo),* and has been produced artificially by Moissan and others with the same metal as matrix. But in eclogite the silica percentage is at least as high as in dolerite, hence it is difficult to understand how so small an amount of carbon escaped oxidation. I had always expected that a peridotite (as supposed by Professor Lewis), if not a material yet more basic, would prove to be the birthplace of the diamond. Can it possibly be a derivative mineral, even in the eclogite? *Had it already crystallised out of a more basic* magma,† which, however, was still molten, when one more acid was injected, and the mixture became such as to form eclogite? But I content myself with indicating a difficulty, and suggesting a possibility, the fact itself is indisputable. that the diamond occurs, though rather sporadically, as a constituent of an eclogite, which rock, according to the ordinary rules of inference, must be regarded as its birthplace.

This discovery closes another controversy, viz., that concerning the nature of the "Hard blue" of the miners (Kimberlite of Professor Lewis), in which the diamond is usually found. The boulders described in this paper are truly water worn. The idea that they have been rounded by a sort of "cup and ball" game played by a volcano may be dismissed as practically impossible. Any such process would take a long time, but the absence of true scoria implies that the explosive phase was a brief one. They resemble stones which have travelled for several miles down a mountain torrent, and must have been derived from a coarse conglomerate, manufactured by either a strong stream or the waves of a sea from fragments obtained from more ancient crystalline rocks.‡ The "washings," a parcel of which I received from

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* P.S.—The Novo Urei meteorite (not wholly iron) was forgotten.

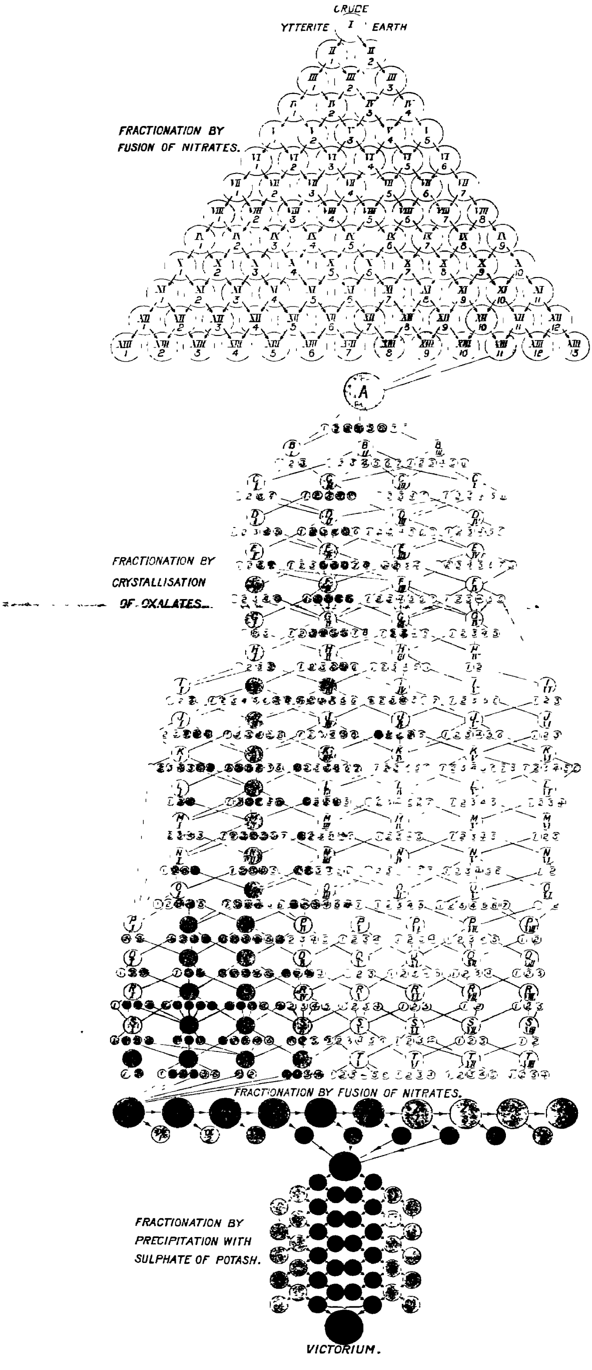
† This, however, cannot have been very rich in iron, because diopside does not contain much of that constituent.

‡ As these eclogites are very coarsely crystalline, we are justified in assuming they were once deep seated rocks, and so much more ancient than the date of the conglomerate. To prevent any misunderstanding I may repeat that the matrix

Mr. Trubenbach, also show that the boulders are really waterworn. Besides two unworn pieces of pyrite and a rough bit of eclogite, about three-quarters of an inch in diameter, the pyroxenic constituent of which was a bright emerald-green (? smaragdite), I find part of a subangular fragment of chromo-diopside associated with two or three flakes of the usual mica, a well rounded garnet fully 0·6 inch across, and half a well worn pebble of eclogite, about one inch long and half an inch thick. The rounded waterworn look of the great majority of the smaller constituents (chiefly garnets and pyroxenes), about the size of hemp seed, is very obvious. I had suspected some of the grains in washings from the De Beers Mine to have been similarly treated; but here it is indubitable, indeed many of the dark green specimens are so smooth outside that they could only be identified after fracture. The ordinary diopside can, however, be recognised, with some of a clearer and brighter green. Most of the garnets are pyropes, but a few resemble essonite. I find also some grains of iron oxide and of vein quartz Thus, the presence of waterworn fragments, large and small, in considerable abundance, shows the "blue ground" to be a true breccia, produced by the destruction of various rocks (some of them crystalline, others sedimentary, but occasionally including waterworn boulders of the former)—*i e*, a result of shattering explosions, followed by solfataric action Hence the name Kimberlite must disappear from the list of the peridotites, and even from petrological literature, unless it be retained for this remarkable type of breccia

Boulders, such as we have described, might be expected to occur at the base of the sedimentary series, in proximity to a crystalline floor The Karoo beds in South Africa, as is well known, are underlain in many places by a coarse conglomerate of considerable thickness and great extent, called the Dwyka conglomerate, which is supposed to be Permian or Permo-carboniferous in age. It crops out from beneath the Karoo beds at no great distance from the diamond-bearing district, and very probably extends beneath it. If this deposit has supplied the boulders, the date of the genesis of the diamond is carried back, at the very least, to Palæozoic ages, and possibly to a still earlier era in the earth's history

from which those boulders were taken (at various depths, from nearly 100 to about 300 feet) cannot be any alluvial deposit, but is the typical "blue ground," practically identical with that in the Kimberley mines


CRUDE
YTTERITE I EARTH
FRACTIONATION BY FUSION OF NITRATES.
A
FRACTIONATION BY CRYSTALLISATION OF OXALATES.
FRACTIONATION BY FUSION OF NITRATES.
FRACTIONATION BY PRECIPITATION WITH SULPHATE OF POTASH.
VICTORIUM.

water, and the liquid well boiled A white precipitate of basic nitrate forms, while the undecomposed nitrates remain in solution. These are separated by filtration, the precipitate going to the right and the solution to the left The basic nitrate is dissolved in nitric acid, and the right and left solutions are then evaporated to dryness and fused as before Partial decomposition by heat again divides each of these portions into two lots, soluble and insoluble The soluble from the left-hand lot goes still further to the left, and its insoluble portion to the right The soluble from the right-hand portion goes to the left, where it mixes with the insoluble from the other portion, while its insoluble goes still further to the right This series of operations is continued for as long a time as the material will hold out * From a description the process seems to be more complicated than it really is, but a study of the diagram and the direction of the arrows make it clear The number of times this operation is performed varies with each lot of earth fractionated The portions submitted to fusion rapidly diminish in quantity, and the operation is continued until the material becomes too scanty

The last horizontal line of fractions spectroscopically examined in a radiant matter tube shows differences in the visible spectrum. For many years I recorded these differences in coloured drawings, which have served on several occasions to illustrate papers before this Society † In the year 1893 I commenced to record the differences between the various spectra by photographing them in a spectrograph having a complete quartz train, and since that time attention has chiefly been directed to the variations in the number, character, and positions of the lines and bands in the ultra-violet spectrum these are more striking than those which are visible, and as they are self-recording, results are more rapidly attained. A description of this instrument is given further on

On placing the photographed spectra of one of the horizontal lines of earths in order, several differences are detected One striking difference is seen in the behaviour of a group of lines in the ultra-violet. It is nearly absent in the end fractions, gradually becoming stronger towards the middle, and attaining a maximum in the fractions situate about two-thirds towards the right. This shows that at least three different bodies are present one, the great bulk, having a nitrate difficult to decompose , another whose nitrate is easiest to decompose , and a third body, occupying an intermediate position, whose nitrate decomposition occurs at temperatures between that required by the others, but nearer that of the nitrate easiest decomposed.

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* "On the Methods of Chemical Fractionation," British Association, Birmingham Meeting, 1886, 'Chemical News,' vol 54, p 131.

† "On some New Elements in Gadolinite and Samarskite detected Spectroscopically, 'Roy. Soc Proc ,' No. 245, 1886, vol 40, p 502."

The above method of fractionation is not so effectual if more than two bodies are present. In that case the process fails, in any reasonable time, to yield practically pure specimens of more than two out of a group of closely allied earths Thus, if there are three earths—say, A, B, and C—whose positions in reference to the chemical process employed are in the written order of sequence, we may get a specimen of A as nearly as we please free from B and C, and a specimen of C as nearly as we please free from A and B, but we cannot get a specimen of B practically free from A and C The law seems to be that to obtain practically pure specimens of three closely allied earths, it is essential to have recourse to at least two different chemical processes The mere continued repetition of the same process will not do, unless indeed the operations are repeated such a vast number of times as to make the approximate expressions no longer applicable, even though the substances are chemically very close

For this and other reasons it is advisable to change the method of fractionation after one process has been in operation for some time. It is evident that any process of fusion, crystallisation, or precipitation can only divide the mass of material into two parts, a soluble and insoluble portion, crystals and mother liquor, and after a time a balance of affinities seems to be established, and further fractionation appears to do little good It is better then to change the operation.

Following the diagrammatic scheme, the portions of earths containing most yttrium are collected together and fractionated by the crystallisation of the oxalates from a solution strongly acidulated with nitric acid in the following manner —

To a boiling acid solution of the nitrate a small quantity of hot solution of oxalic acid is added. The solution remains clear, and it is only after vigorous stirring that a small quantity of insoluble oxalate is formed The whole is thrown on a hot-water filter and slightly washed with boiling water To the boiling filtrate a fresh lot of hot solution of oxalic acid is added, and stirred till more insoluble oxalate comes down. This is again filtered off, and the operations of precipitating, stirring, filtering, and washing are repeated, always keeping the temperature as near the boiling point as possible, until the whole of the earths are precipitated Generally the initial earth is divided by this method of fractionation into from six to twelve portions. Each of these oxalates is dried, ignited, dissolved in nitric acid, and the above-described operations repeated. Photo-spectroscopic tests are constantly taken during the progress of this fractionation, and portions are mixed together according to the data thus obtained, as shown on the diagram by the lines joining the fractions, the object being to avoid lateral spreading as much as possible, and, while concentrating the special line-giving earth, to prevent its too great diffusion over a large number of fractions. When the fractionation by the oxalate method has proceeded for a considerable time, the fractions rich in victorium are collected together and submitted to another mode of treatment.

These fractions are converted into nitrates, and a small quantity is thrown out by partial decomposition by heat, according to the method already described The filtrate is evaporated to dryness and again fused so as to throw out a little more. This operation is repeated as long as any soluble nitrate is left. Generally from six to twelve portions are thus obtained These form a regular series, differing according to the stability of the nitrate under heat. On testing, the victorium is found to concentrate in the centre portions, being less easily decomposed than the earths of the cerium group and more easily decomposed than those of the yttrium group

The fractions rich in victorium are converted into sulphates and mixed with a hot saturated solution of potassium sulphate. The precipitate is dissolved in boiling water and mixed with a further quantity of solution of potassium sulphate. This produces a small quantity of a precipitate. The filtrate from the first precipitate is also mixed with fresh potassium sulphate, and the operations are repeated, mixing the centre solutions to one lot and the side solutions to another, as shown by the lines on the diagram. It is found on photo-spectroscopic examination that the earths thrown out on each side are poorest in victorium, whilst those in the middle are richest After a time no further concentration is effected in this manner, all the earths that can be removed as being more or less soluble in potassium sulphate having been eliminated

In thus describing the method of fractionation, my object has been not so much to give a description of the plan actually carried out in the laboratory - for the details have varied with each operation—but to give an intelligible idea of the general manner in which a very complicated operation is effected In the diagram I am supposing that one particular substance, victorium, is to be separated, and I have endeavoured to show its migrations and gradual concentration as the work progresses, by tinting the fractions where it mostly would concentrate; the depth of tint representing the amount of concentration.

In the purest condition yet obtained victoria is an earth of a pale brown colour, easily soluble in acids. It is less basic than yttria and more basic than most of the earths of the terbia group. In chemical characters it differs in many respects from yttria. From a hot nitric acid solution victorium oxalate precipitates before yttrium oxalate and after terbium oxalate. On fractional precipitation with potassium sulphate the double sulphate of victorium and potassium is seen to be less soluble than the corresponding yttrium salt, and more soluble than the double sulphates of potassium and the terbium and cerium groups Victorium nitrate is a little more easily decomposed by heat than yttrium nitrate, but the difference is not sufficient to make this reaction a good means of separating victorium and yttrium. Fusing the nitrates can, however, be employed advantageously to separate mixed victoria and yttria from the bulk of their associated earths.

On the assumption that the oxide has the composition Vc_2O_3, the atomic weight of victorium is apparently not far from 117.

The photographed phosphorescent spectrum of victoria consists of a pair of strong lines at about λ 3120 and 3117, other fainter lines are at 3219, 3064, and 3060. Frequently the pair at 3120 and 3117 merge into one, but occasionally I have seen them quite distinct. The presence or absence of other earths has much influence on the sharpness of lines in phosphorescent spectra, and it is probable that these lines will be sharp and distinct when victoria is obtained quite free from its associates.

The best material for phosphorescing in the radiant matter tube is not the earth itself, but the anhydrous sulphate formed by heating the earth with strong sulphuric acid and driving off the excess of acid at a red heat. The sulphate thus produced, probably also containing some basic sulphate, is powdered and introduced into a bulb tube furnished with a quartz window, and a pair of thick aluminium poles sealed into the glass with stout platinum wires. The tube is well exhausted, keeping the current from a good induction coil going all the time. The pumping and sparking must continue until the earth glows with a pure light free from haze or cloudiness, and continues so to glow during the passage of the current without deterioration. The exposure in the spectrograph usually occupies an hour.

I give a diagrammatic plan of the two-prism spectrograph used in this research. It is furnished with two quartz prisms, quartz lenses, and condensers. The slit jaws are of quartz, cut and polished according to the method I described in the 'Chemical News,' vol. 71, p. 175, April 11, 1895.

The prisms are made in two halves, according to Cornu's plan, one half of each being right-handed and the other half left-handed. One of the lenses also is right-handed and the other left-handed. By this device the effect of double refraction is so completely neutralised that with a five-prism instrument it is impossible, under high magnifying power, to detect any duplication of the lines.

The lenses are each of 52 mm. diameter and 350 mm. focus. The focus of the least refrangible rays is longer than that of the most refrangible rays, and the sensitive film must therefore be set at an angle to get the extreme rays into focus at the same time. But this alone is not sufficient. The focal plane is not a flat surface, but is curved, and the film must therefore be curved,* and it is only when

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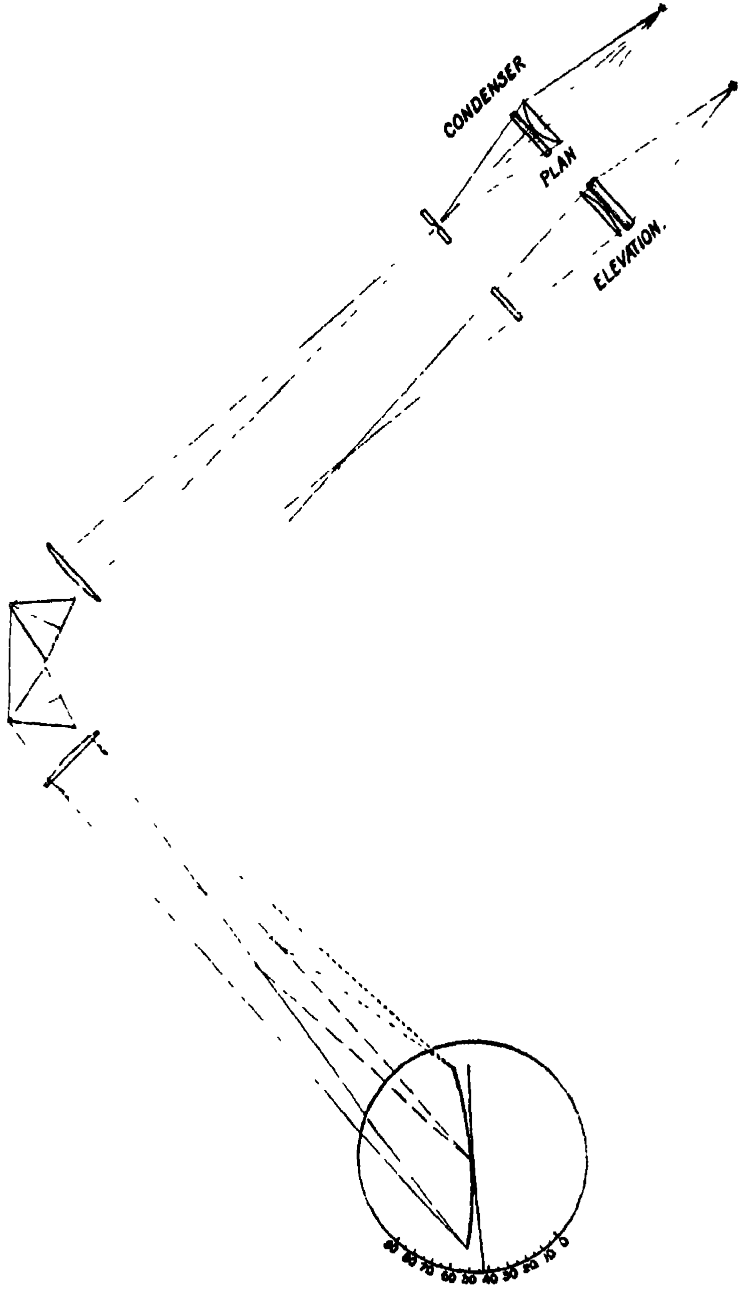
* 'Chemical News,' vol. 72, p. 87, August 23, 1895, and vol. 74, p. 259, November 27, 1896.


CONDENSER
PLAN
ELEVATION.

both these conditions are fulfilled that perfectly sharp images of spectral lines extending from the red to the high zinc line 2138 30 can be photographed on the same surface. Celluloid films are used, glass not being sufficiently flexible.

Using the middle position showing the whole spectrum on a plate, the angle is 40°, and the curvature is 190 mm radius.

The condensers are of quartz, and are plano-cylindrical—one being double the focus of the other. The object of this, when spark-spectra are being photographed, is to concentrate on the slit a line instead of a point of light, as would be the case if ordinary lenses were used.

When photographing phosphorescent spectra—or, in fact, any spectra the wave-lengths of which are either unknown or require verification—I always photograph on the same film a standard spectrum, usually of an alloy of equal molecular weights of zinc, cadmium, tin, and mercury. This forms a hard somewhat malleable alloy, giving throughout the whole photographic region lines the wave-lengths of which are well known. The chief objection to this alloy is its volatility—the poles requiring frequent adjustment. Recently I have used pure iron for this purpose, this has the advantages of giving a great number of fine lines whose wave-lengths are accurately known, and not being very volatile, the poles do not rapidly wear away If the poles are kept about 1 mm. apart there is little or no interference from air lines.

The most simple method of applying the standard lines to an unknown spectrum is by the successive employment of two slightly overlapping diaphragms immediately behind the slit, one being used for the experimental and the other for the standard spectrum. In this way, without disturbing the instrument, the two spectra can be recorded on the plate one over the other, the overlap of 1 mm. being in the optical centre of the train. The resulting negative is then transferred to a micrometer measuring machine of special construction, having a screw of 1/100 of an inch pitch, and a means of accurately determining 1/1000 of its revolution—thus measuring directly to the hundred-thousandth of an inch. In this way, in a five-prism spectrograph having lenses 700 mm. focus, it is possible to determine wave-lengths of photographed lines to the sixth figure.

"Experimental Contributions to the Theory of Heredity. A. Telegony." By J. C. Ewart, M.D., F.R.S., University of Edinburgh. Received May 29,—Read June 1, 1899.

I. *Introductory.*

The belief in telegony, or what used to be known as the "infection of the germ" or "throwing back" to a previous sire, has long prevailed

It may for all we know be as old as the belief in "mental impressions," which has had its adherents since at least the time of the patriarchs. During the eighteenth century the "infection" doctrine was frequently discussed by physiologists, and since Lord Morton, in 1820, addressed a letter to the Royal Society on the subject, believers in "infection" have been increasing all over the world, with the result that one seldom now hears of breeders or fanciers who are not influenced by the doctrine, while physicians and others interested in the problems of heredity either as a rule take telegony for granted or see nothing improbable in the "infection" hypothesis.

It must, however, be admitted that, notwithstanding the criticisms of Weismann and others, very different views are entertained by the believers in telegony, not only as to the cause, but as to the results, of "infection." By some telegony is confounded with simple reversion or atavism, while the better informed generally assume that "infection" invariably results in the subsequent offspring repeating more or less accurately the characters of the first or of a previous sire. In a breeders' journal of some standing there appeared recently under the heading "Colour of Animals" the following sentence —"Greys show in breeding a great tenacity of assertion, as they are few in comparison to other colours in the Stud Book, but they reappear and no doubt go back to the Arab, and prove telegony to be a fact."* This shows simple reversion is sometimes mistaken for telegony. In support of the view that "infection" is commonly supposed to lead to "throwing back" to a previous sire many instances could be given, but the following from an article on telegony by De Varigny will suffice. De Varigny states that an ordinary cat which had kittens to a tailless Manx cat subsequently produced several tailless kittens to a normal cat of her own breed †

An extended series of experiments with various kinds of animals has led me to the conclusion that if there is such a thing as telegony, it is more likely to result in the subsequent offspring "throwing back" to an ancestor of the "infected" dam than to a previous mate. This view of telegony (which has not been insisted on hitherto) will be made at once evident by an example. A sable collie crossed with a Dalmatian produced three pups which in their coloration are extremely like young foxhounds; instead of numerous small spots each has a few large blotches. According to the common view of telegony this collie, if infected, should next produce with a dog of her own breed one or more Dalmatian-like pups. If, however, the offspring of a collie and a Dalmatian are like foxhounds, the subsequent offspring to a collie of the same colour and strain could hardly be expected to present Dalmatian characters, *i.e.*, show numerous small spots. But if "infection" as a rule results in the subsequent offspring "throwing back" either to the ancestors of the sire or the dam, it will be extremely difficult, if not in many cases impossible, to distinguish telegony from simple reversion.*

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* 'Live Stock Journal,' May 12, 1899, p. 588.

† 'Journal des Débats,' September 9, 1897.

But though "infection," if it does take place, is likely, as a rule, to lead the subsequent offspring to resemble the ancestors of the dam, it may in certain cases possibly lead to their "throwing back" to a previous sire. This result might follow if the previous sire happened to be highly prepotent. For example, Highland heifers often produce to a Galloway bull hornless black offspring indistinguishable from pure Galloways If infected by the Galloway bull, these heifers might afterwards produce Galloway-like calves when mated with long-horned bright coloured bulls of their own breed

It is now commonly believed that if there is such a thing as telegony it results from the unused germ cells of the first (or previous) sire infecting - blending with—the unripe germ cells in the ovaries of the dam Were this possible, the subsequent progeny would in all probability in a mild way resemble the previous sire, but if this is impossible, then infection—due perhaps to some obscure change in the constitution or reproductive system of the dam is more likely to lead to more or less marked reversion to the ancestors of the dam. All my observations point to its being impossible in the Equidæ for the unused male germ cells of the first sire to infect the unripe ova. The spermatozoa lodged in the upper dilated part of the oviduct of the mare are dead, and in process of disintegrating, eight days after insemination; they probably lose their fertilising power in four or five days. There is no reason for supposing that in the Equidæ they survive longer in or around the ovary. Further, though at the time of fertilisation there may be several large Graafian follicles in each ovary containing maturing ova, all these follicles disappear long before the period of gestation is completed. The subsequent foals are developed from successive new crops of ova into the composition of which it is inconceivable any of the spermatozoa of the first sire could by any chance enter. A study of the ovaries hence tends to confirm the view that "infection" (if there is such a thing) is as likely to cause reversion to a former ancestor of the dam as a "throwing back" to a previous sire.

Having made these general observations, it will be well next to consider critically the case of "infection" communicated in the letter to the President of the Royal Society in 1820 by the Earl of Morton.

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* That reversion ever occurs has been questioned by Bateson ('Materials for the Study of Variation') and others, but I have already ('Nature,' February 9, 1899) proved beyond doubt that reversion can be easily induced by intercrossing distinct types, and I have recently heard of several instances of spontaneous reversion —reversion not induced by intercrossing.

Though many other instances of supposed "infection" have been recorded, Lord Morton's mare may be said to still hold the field—the theory of telegony still mainly rests on the assumption that this historic mare was "infected" by a quagga some years before she passed into the hands of Sir Gore Ouseley and produced three "colts" to a black Arabian horse. One might even go further and without much exaggeration assert that the telegony hypothesis at the present moment mainly rests on an allegation by Sir Gore Ouseley's stud groom

It has been generally assumed that Lord Morton's mare (a nearly purely bred chestnut Arab) was "infected" for two reasons (1) because the subsequent offspring were of a yellowish-brown colour and more or less striped, and (2) because, according to Sir Gore Ouseley's stud groom, the mane of one of the striped foals had always been upright, while in another it arched to one side clear of the neck The presence of stripes in the subsequent offspring has never been questioned, nor yet is there any doubt that when Lord Morton in 1820 inspected the "colts" the mane in the filly was upright as in the quagga, while that of the colt resembled the mane of Lord Morton's quagga hybrid. There is, however, an absence of reliable evidence that the filly's mane had *always* been upright as alleged to Lord Morton by Sir Gore Ouseley's stud groom

Were the evidence in support of this allegation satisfactory, there would I think be no escape from the conclusion that Lord Morton's mare was "infected" by the quagga Hitherto the presence of stripes on the "colts" has generally been looked upon as affording strong evidence of "infection" Believers in telegony admit that stripes are not uncommon in Norwegian and certain other breeds of horses, but, with Mr Darwin, they have taken for granted that they never or very rarely occur in Arabs

I find, however, that though in Arabia dun-coloured horses are disliked and never used for breeding, stripes even in the most renowned strains are not so uncommon as is generally supposed. I have now a purely bred Arab filly of about the same colour as Lord Morton's filly, but, unlike the filly we have heard so much of, both the fore and hind legs are marked with distinct dark bars, and there are faint indications of stripes across the withers and a distinct dorsal band. The history of this filly (bred by Mr. Wilfred Scawen Blunt at Crabbet Park, Sussex, and very kindly presented to me) is well known for many generations, none of her ancestors could possibly have been "infected" by a zebra. The dun colour and stripes are doubtless the result of simple spontaneous reversion, for, unlike Lord Morton's mare, there is no history of a cross in her pedigree. This filly proves that even in high-caste Arabs of the best desert blood a dun colour and stripes may unexpectedly appear.

As to the occurrence of stripes in other breeds I could give, were it necessary, many instances. A year ago I had in my possession a light bay (or yellow dun) pony, which showed nearly as many stripes on the trunk as the Gore-Ouseley filly, and in addition had several interrupted narrow stripes on the forehead.* Moreover, the stripes on the Gore-Ouseley "colts," while agreeing with stripes occasionally seen in horses, differ in their arrangement from the stripes in the quagga. The stripes themselves are evidence of reversion, but nothing more, and seeing that pure bred horses sometimes show quite as many stripes, we are not justified in assuming that but for the dam of the "colts" having been first mated with a quagga the stripes would not have appeared.

Hence unless it is proved that the mane in the filly and colt were naturally erect, or nearly erect, the case for the "infection" of Lord Morton's mare will be lost. It may be well to quote the passage from Lord Morton's letter referring to the mane It is as follows - "That of the filly is short, stiff, and upright, and Sir Gore Ouseley's stud groom alleged it never was otherwise That of the colt is long, but so stiff as to arch upwards and to hang clear of the neck, in which circumstance it resembles that of the hybrid. This is the more remarkable as the manes of the Arabian breed hang lank and closer to the neck than that of most others "†

I am not prepared to accept the allegation as to the manes for the following reasons ·—

1. I have had twelve zebra hybrids under observation, and in each case the mane, though erect to start with, always after a time arched over to one or both sides The stud groom's statement, it seems to me, proves too much. If in the quagga hybrid and in all my horse hybrids the mane, sooner or later, falls to one side, it is a little remarkable that in the pure bred two-year-old filly it had been always upright.

I may here mention that the hair of the mane of zebra hybrids is shed annually; it is for this reason that the mane in hybrids is never long enough to hang close to the neck

2. The mane in the drawing of the filly by Agassé is not represented as upright, but as lying to one side. If the mane had remained erect during the first two years, by virtue of shedding its hairs, it could not very well have lost this habit and fallen completely over to one side subsequently, say, during the fourth year From the mane being erect in 1820, and hanging to one side in 1821 or 1822, when Agassé's drawing was made, the presumption is that the mane of the "colts" had been cut some time before they were examined by Lord Morton

Two years ago I had a bay Arab with a mane which was to start with short, stiff, and upright, some months later it arched freely to one side, as in my zebra hybrids, and later still it hung lank and close to the neck.

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* See Fig 36, 'The Penycuik Experiments,' A. and C. Black, 1899.

† 'Phil. Trans.,' 1821.

3. There is always an intimate relation in the Equidæ between the mane and the tail, when the mane is short and erect the upper third or so of the tail is only covered with short hairs, which, like the hairs of the mane, are annually shed. Lord Morton noticed nothing peculiar about the tail of the "colts," and the tail of both the colt and filly in Agassé's drawings is the tail of a high-caste Arab This seems to me to warrant the conclusion that the filly's mane had been hogged some time before Lord Morton's visit.

It thus appears that the evidence in support of the belief that Lord Morton's mare was "infected" by the quagga is at the best far from satisfactory. The same may be said of the evidence in support of all the other supposed cases of telegony in the Equidæ—of, amongst others, Lord Mostyn's mare, referred to by Darwin,* of the mule-like mare in the Paris Gardens, referred to by Tegetmeier and Sutherland,† and of the African ass (*Equus asinus*), still in the Zoological Gardens (London), which now and then has a reddish-coloured foal, like the cross-bred foal she produced in 1883 to an Asiatic ass (*E hemionus*).

Although I am now satisfied that Lord Morton's case throws little light on the telegony hypothesis, like many others I had no very decided views on the subject some years ago, and hence when arranging in 1894 to make a collection of horse embryos, I decided to repeat, as far as circumstances permitted, what is commonly called Lord Morton's experiment For this purpose, I procured early in 1895 three zebras and a number of mares. Two of the zebras died during the winter of 1895, but the third—a handsome stallion of the Chapman variety (*E. burchelli* v. *chapmani*) still survives and is now thoroughly acclimatised.

During 1895 I only succeeded in mating the zebra with one mare, and hence there was only one hybrid born in 1896. During the last two years however, quite a number of hybrids have made their appearance, and the dams of several of the hybrids have subsequently produced pure-bred foals. The time has hence come, when some of the results of the experiments may with propriety be communicated to the Royal Society.

"II. Experiments with West Highland Ponies." By LORD ARTHUR CECIL, Orchardmains, Kent, and J. C. EWART.

The first mare mated with the zebra was a black, West Highland pony (Mulatto), set apart for the telegony experiments by Lord

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* 'Animals and Plants,' vol. 1, p. 435, 1875.

† 'Horses, Asses, and Zebras,' p. 81.

Arthur Cecil. The better bred West Highland ponies are supposed to have descended from "Armada" horses, and are hence perhaps related to Mexican and Argentine horses, so often dun-coloured and partially striped. Mulatto's hybrid (Romulus, born 12th August, 1896) is on the whole more a zebra than a pony both mentally and physically. He is especially remarkable in being more profusely striped than his sire (the zebra Matopo), in having a heavy semi-erect mane, which is shed annually, and in having a mule-like tail, from the upper third of which the longer hairs are periodically shed. The body colour of the hybrid varies from a dark orange colour to a mouse dun; the stripes of a reddish-brown colour on the head are dark brown or nearly black on the trunk and limbs

In the number and plan of the stripes, the hybrid agrees more closely with the Somali zebra than with any of the Burchell zebras. Over the brow, *e.g.*, there are narrow rounded arches instead of somewhat broad pointed arches as in his sire, the neck and trunk have quite double the number of stripes found in the sire, while over the croup in the position of the "gridiron" of the mountain zebra there were at birth irregular rows of spots which in course of time blended to form somewhat zig-zag, narrow, transverse bands The ears are nearly as large as in the sire, while the eye-lashes are longer and distinctly curved In his movements the hybrid resembles his sire, and like his sire he is always on the alert, very active and suspicious of unfamilar objects. Further in his call he agrees far more with his sire than his dam. In being profusely striped, Romulus differs greatly from the quagga hybrid bred by Lord Morton, in which the stripes were fewer in number than in many dun-coloured horses.

Mulatto's second foal arrived in July, 1897, the sire, Benazrek, being a high-caste grey Arab horse Like Lord Morton's colts, Mulatto's foal by the Arab horse, in make, action, and temperament, agreed with ordinary foals, but it differed from the majority of foals in presenting quite a number of *indistinct* stripes—subtle markings only visible in certain lights. These stripes differed but little from the body colour, which varied from dark bay to brown. Though few references have been made to the occurrence of stripes in foals, they are, we find, far from uncommon. As is well known, Mr. Darwin once bred a striped foal by putting a cross-bred bay mare to a thoroughbred horse. This foal was for a time marked nearly all over with obscure dark narrow stripes, plainest on the forehead, but also distinct over the croup *

There is no figure of Mr Darwin's striped foal, but from the description given, there can be little doubt that the markings were more abundant than in Mulatto's second foal In this foal (as in Mr. Darwin's) the stripes became more and more indistinct, and by November they had almost vanished. Unfortunately, the foal died when about five months old, and hence it is impossible to say whether any of the stripes would have persisted It will be evident that Mulatto's second foal helped but little to clear up the vexed "infection" problem Mulatto missed having a foal in 1898, but she recently produced at Knole, Kent, her third foal The sire (Loch Corrie) of this foal belongs to the Island of Rum section of the West Highland ponies, and closely resembles Mulatto The third foal has about as many stripes as the second. As in the second, they are most distinct over the croup and hind quarters, and as in the second, they differ both from the markings in the previous sire, the zebra, and from the markings of the hybrid Romulus.

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* 'Animals and Plants,' vol. 1, p 60.

This third foal, which was born on the 6th of May, 1899, seemed like the second, to lend some support to the "infection" hypothesis Fortunately, since it made its appearance, two other West Highland mares have had foals to Loch Corrie These foals put all doubt as to the nature and significance of the stripes on Mulatto's second and third foals at an end.

One of the dams is of a brown colour, the other is nearly black Though neither the brown dam nor the black has ever seen a zebra, both foals are marked in very much the same way as Mulatto's, and some of the stripes in one of the new foals look more like persisting than the stripes on Mulatto's third foal Hence, in order to account for the markings on Mulatto's foal to the grey Arab, and on her foal to the black West Highland pony, it is unnecessary to fall back on the "infection" hypothesis

"III. Experiments with Shetland, Iceland, Irish, Thoroughbred, and other Ponies" By J. C Ewart

An effort was made to cross four Shetland ponies with the zebra stallion, but I only succeeded in obtaining one hybrid. The dam (Nora) of this hybrid closely resembles, except in size, the Island of Rum ponies—she is a small edition of Mulatto. Her first foal, by a black Shetland pony, was of a dun colour and nearly as striped as Sir Gore Ouseley's filly; her second is the most zebra-like of all my hybrids, her third closely resembles her sire, a bay Welsh pony. For some time after birth there were faint indications of stripes over the hind quarters of this foal, but now it is a year old there are no markings or any other suggestions of a zebra It is not a little suggestive that the foal bred from this pony before she was mated with the zebra was distinctly striped, while the subsequent pure bred foal has no stripes.

Of five Iceland ponies put to the zebra only one produced a hybrid. This hybrid was faintly striped, and showed less of the zebra than any of the others. The dam, a prepotent yellow and white (skewbald) pony, had first of all a light bay foal to an Iceland pony. Her third foal, by a bay Shetland stallion, is a skewbald, and in the size and arrangement of the brown patches closely resembles the dam. There is no hint whatever that the Iceland pony has been "infected" by the zebra

Several Irish mares were put to the zebra, and two of them (bays) have first produced hybrids and subsequently pure bred foals A cream coloured Irish-Canadian mare unfortunately died before her hybrid foal was born. One of the bay mares had a bay hybrid richly striped; the other a hybrid with but indistinct stripes. The subsequent foals—one by a chestnut thoroughbred horse (Tupgill), the other by a hackney pony (Mars Royal)—are bays, not only devoid of stripes but affording no indication whatever that their dams had been previously mated with a zebra.

Although I experimented with seven English thoroughbred mares and an Arab mare, I only succeeded with one, a small chestnut. This mare produced twin hybrids last summer, yesterday she had a foal to a thoroughbred chestnut horse (Lockstitch) One of the twins died soon after birth, the other, richly but unobtrusively striped, in its colour and make strongly suggests its dam. The chestnut mare's new foal neither in make, colour, nor action in any way resembles a young zebra nor a zebra hybrid In 1897 a bay mare by a bay Arab horse (Hadeed) was for some months in foal to the zebra Since she miscarried in 1896 she has had two foals to a thoroughbred horse (Lockstitch) Neither of these foals in any way suggests a zebra. In this case the unused germ cells of the zebra had presumably a better chance of reaching the ovum from which the first of the two pure-bred foals was developed than is usually the case.

Attempts were made to cross Welsh, Exmoor, New Forest, Norwegian, and Highland ponies with the zebra without success, and though a cross-bred Clydesdale has twice had a hybrid, she has not yet produced a pure bred foal. The experiments as far as they have gone afford no evidence in support of the telegony hypothesis.

June 8, 1899.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

Meeting for Discussion.

Subject —Preventive Inoculation.

Professor William Osler (elected 1898) was admitted into the Society.

Mr Henry J H Fenton, Dr. Henry Head, Professor Conwy Lloyd Morgan, Mr Clement Reid, Professor H. S Hele-Shaw, Dr. Ernest Henry Starling, Professor Henry W. Lloyd Tanner, and Professor B. C. Allen Windle, were admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Discussion was opened with the following communication —

"On Preventive Inoculation"* By W M. HAFFKINE, C.I.E Communicated by LORD LISTER, P R S. Received June 5, —Read June 8, 1899

My Lord and Gentlemen,—The most important modern methods of prophylactic treatment are based upon the fact that an attack of disease from which an individual recovers leaves in him a condition of relative immunity to another attack.

The method of turning this result to advantage for the protection of whole communities was first demonstrated to us by Mahomedan physicians, to whom the world thus owes what proved to be one of the most fertile principles of modern science.

The successes of Jenner and Pasteur, who utilised cultivated virus for preventive treatment, have led to a general conception that there is the possibility of creating artificial immunity to diseases by treating the organism with morbid virus rendered by some special means harmless.

This view involves a generalisation which led to a considerable amount of disappointment, as the application of the principle in a number of instances did not give the expected results.

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* The paper as published here, after final revision by the author, differs in some details from what has appeared in the 'Lancet' of June 24, and 'British Medical Journal,' of July 1, 1899, under the title of "A Discourse on Preventive Inoculation, delivered at the Royal Society, London, on June 8, 1899."

Derivatives from Microbic Virus and their Effect

When we cultivate a pathogenic micro-organism in a liquid medium, two different elements are obtained mixed together the bodies of the microbe and the liquid which it has modified, and into which it has secreted its own products.

A modification of the entire preparation, as represented by this mixture, can be first of all obtained by filtering and separating the two elements just mentioned, and considering each of them by itself

Or else the two can be left together, and only the vitality of the microbe destroyed by some physical or chemical agent

Or the constitution and the properties of each, or of both of these elements can be, to a desired degree, altered by the admixture of chemicals, or by subjecting them to physical processes

Or, the vital and pathogenic properties of the microbe can be modified by artificial breeding, and then the microbe itself, or the products of such a modified microbe, used for treatment.

The immediate effect which a given virus or its derivate produces on an animal differs with the kind of virus taken, the process of modification to which it has been subjected, and the species of animals upon which it is used. The following instance may give an idea of these variations.

The ordinary Indian grey, as well as the brown, monkey are susceptible to the plague virus, and may contract a fatal disease from being simply pricked with an infected needle The rabbit and guinea-pig are also susceptible to the disease The horse, on the contrary, contracts no fatal disease after being infected even with large doses of the living virus.

If, however, a plague culture be heated and the microbes killed in it, the relative susceptibilities of the monkey and the horse seem to be reversed, the guinea-pig remains comparable with the monkey, while the susceptibility of the rabbit is now like that of the horse, and not, as previously, like that of the monkey.

It will require, according to several observers, a very large dose of virus so treated to produce in the monkey or in the guinea-pig any marked rise of temperature, or any alteration of the skin at the seat of injection; while the horse answers to the injection by almost as brisk an attack of fever as if the virus were a living one, and at the seat of inoculation a tumour is produced which, if the dose be at all considerable, may lead to a complete mortification of the tissue. The rabbit similarly answers to the injection by an attack of fever and by the formation of a hard tumour at the seat of injection.

As varied as is the immediate effect of different forms of virus upon different animals, so varied is the result of the application of such virus from the point of view of immunity conferred by it.

There are animals in whom the inoculation leaves no lasting effect whatever. In others a very temporary immunity is created, vanishing entirely in a few days. In other cases again, a condition appears that produces the impression that, after the treatment, the animal has become more susceptible to a subsequent infection than is a normal animal not so treated

And lastly, there may be found animals in which the same virus will produce a firm and enduring immunity

In general I believe it to be admissible that in the case of every disease, and with regard to every species of animal, a form of prophylactic treatment may be found that will produce immunity in that particular case, but that same method of treatment may or may not be applicable to another animal or to another disease affecting the same animal

It is the failure of taking into account this variation of circumstances that, I believe, more than anything else has checked the success of a number of experimenters.

Immunity from Attack, and Resistance to actual Symptoms of Disease.

The study of the anti-cholera inoculation in India has revealed a new problem in the subject of prophylactic treatment

The particular character of cholera epidemics, which appear unexpectedly, do not last, and in places where they are permanent, are spread and scattered over large areas, makes the study of that disease and the demonstration of the effect of a preventive treatment in its case a matter of much greater difficulty than is the case in localised contagious diseases, like smallpox, or in plague, and although a large amount of material has been collected already, it is desirable that further observations be still added to the present ones confirmatory of the results obtained.

The information collected permits, however, already of pointing out very important features in the working of the anti-cholera inoculation.

The most extended and continuous observations on the subject were organised by the Municipality of Calcutta, upon the enlightened initiative of Professor W J Simpson, late Health Officer, and under his continuous supervision, as well as my own. These observations refer to the cholera stricken suburbs of Calcutta, the so-called "busties" or groups of huts situated round the tanks, where rain-water is collected during the monsoon.

Some 8000 people were inoculated in those localities, and for two years observations were made and the results collected as to the occurrences of cholera in the huts inhabited by the inoculated.

In the vast majority of cases there lived in the same families members who had not been inoculated, together with others inoculated, and the possibility thus presented itself of comparing the incidence of the disease in individuals of the same households, exposed as much as it is possible to the same chances of infection.

During the time under observation cases of cholera occurred in seventy-seven huts. The interval which elapsed between the application of inoculation in each particular hut and the occurrence of cholera in it was as follows:—

Among *uninoculated* members of the families cholera occurred—1, 2, 3, 4, 5, 6, 9, 12, 13, 15, 17, 22, 34, 37, 44, 57, 62, 63, 71, 95, 99, 109, 114, 118, 119, 120, 129, 132, 139, 143, 162, 189, 191, 203, 240, 251, 271, 281, 284, 300, 309, 318, 319, 334, 356, 359, 362, 370, 372, 378, 383, 384, 389, 391, 393, 394, 401, 404, 408, 416, 433, 446, 448, 453, 472, 493, 498, 673, 720, 723, 724, and 738 days after the inoculation of the other members of these families. Among the *inoculated* members of the families cholera occurred—0, 2, 3, 4, 219, 421, 459, 512, 688, 735, and 738 days after their inoculation.

Thus for a period of 738 days, cases of cholera occurred among the uninoculated, so to say, at all intervals after the date of inoculation, whereas the figures referring to the inoculated showed a striking variation of the incidence when compared at various distances from the time of inoculation. Cases continued to occur among the inoculated for a period of four days after the treatment, and then for 416 days they practically remained free from the disease, only one death from cholera having occurred among them during that time. From the 421st day up to the end of the observations six cases occurred among them again.

The relative immunity in the inoculated considered separately during those three periods shows that during the first four days the inoculated had proportionately 1·86 times fewer deaths from cholera than the uninoculated.

During the period between the 5th and 420th days, *i.e.*, for a period of nearly fourteen months, the number of deaths among the inoculated was 22·62 times smaller than amongst the uninoculated. And for the rest of the time under observation the proportion in their favour fell to 1 to 1·54.* The plan has since been formed of trying the effect of larger doses and of stronger vaccines, in order to obtain a more lasting protection.

While thus the absolute number of cases and deaths from cholera appeared so strikingly influenced by inoculation, the peculiarity that became apparent from the observations in Calcutta as well as in other places was that the proportion of deaths to cases was not changed by the treatment.

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* *Vide* Health Officer's Report to the Chairman of the Calcutta Municipal Corporation, reprinted in the 'Indian Medical Gazette,' vol. 31, No. 8, August, 1896.

Thus, in the observations made in a camp of coolies of the Assam-Burmah Railway Survey, out of thirty-three attacked among the uninoculated portion of the camp twenty-nine died, and of four attacked among the inoculated all four died.

In the Durbhanga prison out of eleven uninoculated attacked all eleven died, while five inoculated attacked lost three.

In the Gaya gaol twenty uninoculated attacked lost ten, and eight inoculated attacked lost five

In a group of tea plantations in Assam 154 cases in uninoculated had sixty deaths, fifteen cases in inoculated had four.

In the East Lancashire Regiment in Lucknow 120 uninoculated attacked had 79 deaths, and 18 inoculated attacked had 13.

This circumstance, the non-reduction of the case mortality by a treatment which influenced unmistakably the case incidence, appears an astonishing divergence from the result of small-pox vaccination, where both the number of attacks and their fatality are reduced by the treatment.

The new aspect of the problem of preventive inoculation which thus presented itself in these observations on human communities consisted in the possibility of a prophylactic treatment being directed separately towards the reduction of the number of attacks, leaving the fatality of the disease unchecked, and towards the mitigating of the character of the disease and the reduction of the case mortality in those who get attacked.

Possible relation between Immunity from Attack and Resistance to actual Symptoms of Disease.

In analysing the nature of this particular result, the following two facts well known in laboratory practice presented themselves to me as of essential significance

In patients who recover from an infectious disease the pathogenic microbe does not disappear from their body for a considerable time after their recovery It does not do them harm any longer, though when transferred to another animal it may still cause a fatal attack. Similarly, as in the case, for instance, of a guinea-pig inoculated with the bacillus of chicken cholera, a naturally immune animal can breed for weeks, in an abscess, microbes of an intense virulence without in the least suffering in its own general health.

A condition seems to set in in the convalescent patient, or to exist in naturally immune animals, by virtue of which they do not suffer from the results of activity of a pathogenic microbe, *i.e.*, from its morbid products; and from that time the presence of the microbe in the system, even in the tissues, becomes innocuous. Immunity against morbid symptoms generated by the products of microbes does not seem to imply necessarily the ridding of the system of such microbes. It is known now, since the discoveries of Behring and Kitasato, that such a resistance to these products can be originated artificially, by gradually treating the system with increasing quantities of toxines The system reacts by developing anti-toxines tending to neutralise the effect of the toxines

On the other hand, Gamaleia first drew attention to the fact that it is possible to create in an animal resistance to lethal doses of virulent microbes without that animal acquiring any resistance to a dose of the products prepared from the same microbes in the laboratory.

One seems justified therefore in considering separately two kinds of immunity One against the living microbe, which would prevent it from entering the system and causing an attack; and another against the fatality of the symptoms of the disease caused by the products of the microbe when the latter overcomes the initial resistance and does invade the system.

In the inoculation against cholera, which is done with the bodies of microbes, the first result alone is obtained

These considerations were confirmed by a set of laboratory experiments by Pfeiffer and Kolle, intended to verify our Indian results, and in the course of which they detected in the serum of men inoculated with only one dose of cholera vaccine an extremely high protective power, equal to that which, in goats for instance, could be created only after a very prolonged treatment, extending over five or six months, and including injections with gigantic doses of cholera vaccine.

On analysing, however, in detail the properties of that serum they found that it possessed an intense power of destroying the cholera microbes, but exhibited no antitoxic properties capable of neutralising the effect of the products of those microbes.

The Plan of Anti-plague Inoculation.

When, in 1896, I was confronted with the problem of working out a prophylactic treatment against the plague, I determined to put to test the ideas originated by the observations on our cholera patients, and to attempt, in the new preventive inoculation, to obtain at once a lowering of the susceptibility to the disease, and a reduction of the case mortality.

This I resolved to obtain by treating the system with a combination of the actual bodies of microbes and of the concentrated products of their activity.

In presenting the above considerations, I beg that they may be considered as provisional, subject to modification or to complete refutation.

There may exist already facts unknown to me, which are opposed to the guesses implied in them. It was those guesses which led to the results obtained in the plague inoculation, but, in giving the reasoning which I passed through while working out the method, I am yielding only to a demand expressed to me to that effect, as I considered that part of my communication unnecessary, the more so that the theoretical conjectures above enumerated are not shared even by very eminent experimenters, such as Pfeiffer himself, to whose results I owe some of my premises; and the correctness of the composition of the plague prophylactic, with regard to the extracellular toxines which I have added to it, the so-called supernatant fluid of the plague prophylactic has been the subject of an animated dispute.

It is certain that no theoretical views conceived by one experimenter are binding, or need even be interesting, to others. What is obligatory is the acceptance of the results obtained.

The Plague Prophylactic.

In order to accumulate for the plague prophylactic a large amount of extra-cellular toxines, the bacilli are cultivated on the surface of a liquid medium where they are suspended by means of drops of clarified butter or of cocoanut oil.

The bacilli grow down in long threads into the depth of the liquid, and produce what I have termed a stalactite growth in broth, an appearance quite peculiar to this microbe, and which, I hope, will be accepted as the specific diagnostic feature of this microbe

The products of their vital exchange—the toxines—are secreted by the stalactites into the liquid and accumulate there.

The growth is periodically shaken off the surface of the broth, after which a new crop appears underneath that surface.

Thus a large quantity of the bodies of microbes is collected at the bottom of the cultivation vessel, and the liquid itself gets gradually permeated with increasing quantities of toxines.

The process is continued for a period of five to six weeks, at the end of which the bodies of the microbes become extremely deteriorated.

It will be seen from this that, in my eagerness to put to test our ability to influence the case mortality I may have, perhaps, paid less attention than I might have done to the problem of reducing the number of attacks; and I have now sketched out a simple plan whereby to test this circumstance, and to try to improve our results from this point of view.

In order to render harmless the inoculation of the virus above described, I determined to kill the microbes by heating the material up to 65 to 70° C. The virus so treated, differing from what is observed in some other instances, loses at once, for the animals susceptible to the disease, almost all its pathogenic power; and it was a question to determine, whether it contained the qualities that were sought for, viz., the power of creating *in man* a useful degree of resistance to plague.

The plan has been contested by a number of experimenters who tried a material similarly prepared on different animals and failed to detect in it any immunising properties.

Among other forms of plague virus which were tested by us, and by other experimenters, a large number were found to be too dangerous to use, in other instances the mode of application was inadmissible in the case of men, in others again, the effect appeared too transient to be of practical use

The Properties of the Plague Prophylactic.

The immunising effect of the plague prophylactic, as above described, was worked out on domestic rabbits, and its actual efficiency was verified and confirmed by a number of investigators who experimented on the infection with virulent plague of protected and unprotected rabbits.

Comparing the rabbit with other laboratory animals, such as the rat, the guinea pig, the mouse, the monkey, one may consider the rabbit as the one that perhaps required the least amount of protection, as its natural resistance to plague is relatively high. The most altered virus, *i.e.*, such as was rendered the most harmless of all, was found to confer on the rabbit a very considerable degree of immunity, enabling it in a few days to resist ten or fifteen-fold lethal doses of virulent plague microbes The same treatment applied to animals of a more susceptible nature would, on the contrary, in many instances fail.

The Questions which were to be solved by Experiments on Human Beings.

At the end of our laboratory experiments a set of questions stood before us that were to be solved by direct experiment on human beings. Those questions were —

1. Would man behave with regard to the prophylactic like the animals upon which its protective power had been worked out?
2. If it so happens that the answer is affirmative, what would the dose of the prophylactic, and the method of administering it be; and would not the dose required be so high, and the reaction to be produced so severe, or the number of inoculations to be repeated so great, as to render the treatment inapplicable to men, or impracticable?
3. How many days counting from the date of inoculation would it take to produce in man a useful degree of immunity?
4. How long would that immunity last?

And lastly, there followed two questions, to which my experience of the anti-cholera inoculation entitled me to give a reassuring answer, but the correctness of which it was necessary to demonstrate afresh in the case of plague, viz

5. During the period of reactionary fever and all the other symptoms produced by inoculation, will the resistance of the inoculated exposed to plague be, for the time being, reduced, or remain the same, or be increased? i.e., would it constitute a danger to apply the inoculation in localities actually affected with plague? and

6. When a man who happens to be incubating the plague, or to have initial symptoms of the disease already, chances to be inoculated, would it aggravate his condition, or have no effect, or on the contrary, help him?

Demonstration of the Harmlessness of the Treatment

The perfect harmlessness of the inoculation was first of all demonstrated by the officers of the Laboratory, the Principal and Professors of the Grant Medical College, a large number of leading European and native gentlemen of Bombay, and their families and households, being inoculated And then, in the last week of January, 1897, when the plague broke out in Her Majesty's House of Correction at Byculla, in Bombay, the option of inoculation was offered to the prisoners.

The Experiment in Her Majesty's Byculla House of Correction, Bombay.

The Byculla House of Correction is a long-term prison. There are no children, nor very young people among the inmates, there being in Bombay a separate establishment, the Sassoon Reformatory, where minor criminals are sent.

The prisoners of the House of Correction present a well-fed, well clad, regularly worked, and almost as uniform a set of people as can be seen in a regiment, amongst whom one could scarcely see a single infirm or very aged individual.

At the appearance of plague the prisoners numbered 346 souls.

The inoculation was introduced after nine cases of plague had already occurred, five subsequently ending fatally; there remained thus 337 individuals to be dealt with. Of these, 154 only volunteered for inoculation, and 183 remained uninoculated.

On the 30th January, in the forenoon, before the inoculation was applied, six more cases occurred, of which three afterwards proved fatal. The inoculation was applied in the afternoon, and afterwards it was discovered that one more prisoner had already a bubo on him when inoculated, while two prisoners developed buboes the same evening after their inoculation. These three cases, attacked on the day of inoculation, proved also fatal.

After that, the difference which was observed in the fate of the two groups, the inoculated and uninoculated, is seen from the subjoined table:—*

Date of occurrence of plague	Occurrences in uninoculated			Occurrences in inoculated		
	Number of uninoculated present	Cases	Fatal.	Number of inoculated present	Cases.	Fatal
1897. 23rd to 29th January, previous to the day of inoculation	..	9	5			
30 1 97, the day of inoculation. Forenoon, before inoculation	..	6	3			
Afternoon, after inoculation	..	..	..		3	3
1st day after inoculation, 31.1 97.. ..	177	2	1	151	1	
2nd day after inoculation, 1 2 97	172	1	1	150		
3rd day after inoculation, 2 2 97....	173	1	1	146		
5th day after inoculation, 4 2 97......	171	1	1	146		
6th day after inoculation, 5 2 97.......	169	2	1	146		
7th day after inoculation, 6 2 97......	169	5	1	146	1	
Total after the day of inoculation	172 uninoculated, average daily strength.	12 cases	6 deaths	147 inoculated, average daily strength.	2 cases	No deaths

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* During the time under observation the number of *uninoculated* was reduced on the second, fifth, and sixth day after inoculation by three, one, and one discharged prisoners, whose terms of confinement expired on those days, and it was increased on the third and seventh day by two and two prisoners newly admitted into the jail. The number of *inoculated* was reduced on the third day after inoculation by four released prisoners.

No information as to the subsequent history of these few released (uninoculated and inoculated) prisoners reached the authorities or the officers of the Laboratory, but the question had no interest for the experiment, since their conditions of life and their exposure to plague ceased to be comparable from the time the prisoners were discharged into a large city, with various chances of infection, different in its different quarters. The observations referred only to the prisoners who remained exposed to plague under the conditions of the jail

With the exception of the fourth day, cases of plague continued to occur among the uninoculated group for seven days after inoculation, their average daily strength throughout the week being 173; altogether twelve cases occurred among them with six deaths, while in the 148 inoculated there was one case on the next day after inoculation, who rapidly recovered, and one on the last day of the epidemic, who recovered also.

Analysis of the Results of the Byculla Jail Experiment.

A glance at the above table will show the progress which was made in our information by that initial experiment, and how far it carried us ahead from the state of uncertainty which surrounded the question originally

The dose of prophylactic administered to the prisoners was 3 c.c.

They all had the customary attack of fever from the operation, with the discomfort accompanying that condition,—a headache in many cases, nausea, loss of appetite for a couple of days, a feeling of fatigue and lassitude, recalling a mild attack of influenza, with swelling and pain in the inoculated side. Did, however, all this make them more susceptible to the disease than were their non-inoculated fellow inmates? It is certain that the results testify unmistakably to an opposite effect

Further, the incubation period in plague appears to be on the average five days, extending, however, not unfrequently up to ten.

As to the few newly admitted (uninoculated) prisoners mentioned above, none were subsequently attacked, and the cases of plague stated in the table refer all to old residents, who were present in the jail on the day, and long before the day, of inoculation. Had a case of plague occurred in any of the uninoculated new-comers, it would not have been included in the comparison with the occurrences among the inoculated inmates, since such new comer had been exposed outside the jail to conditions of infection other than those which existed inside the jail. Notwithstanding this, it was permitted that the number of the uninoculated new-comers should be added by the jail authorities to the strength of the uninoculated as quoted in the table, since this tended to weaken, and not to exaggerate, the proportion of immunity obtained by inoculation

In a similar way, in the outbreak of plague in the second (Umerkadi) Bombay jail, described below, one of the three attacks among inoculated persons, as reported by the jail authorities, namely in a man named Sital Vary, prisoner No. 7542, occurred after he had been set at liberty in the city. Had it been an attack in an uninoculated person, it would not have been admitted into the comparison with the inoculated, for the reason mentioned above, *i e.*, because the conditions of infection outside the jail were not comparable with those in the jail But as this was a case in an inoculated subject, and tended to present the results in a less favourable light, it was included in the record

Every prisoner attacked with plague and taken away to the isolation or plague hospital was excluded on the subsequent day from the strength of those who remained in the jail and were still liable to an attack.

Of the twelve prisoners in the uninoculated group who developed plague during the next few days after the date of inoculation a large proportion, if not all, must have been already incubating the disease on that day; and seeing the perfect similarity of conditions under which the inoculated and the uninoculated, who belonged to the same crowd of people, were living, one could infer safely that a similar group of individuals incubating plague was present among the inoculated also at the time when the inoculation was performed on them.

The inoculation, however, did not aggravate their condition, as the number of inoculated who developed plague, counting from the first twelve hours of inoculation, was proportionately five times smaller than the corresponding number among the uninoculated, and the two cases that appeared among the inoculated, one on the very next morning after inoculation, both ended in recovery

As far as that first experiment went, therefore, men behaved like the laboratory animals upon which the prophylactic properties of the drug had been worked out.

For communicating that protection one injection of prophylactic appeared sufficient, with a dose of 3 c.c., which dose, however, in our subsequent operations was further reduced to 2½ c.c

The difference in favour of the inoculated appeared within some twelve hours after the operation, but the man who was inoculated with plague on him, and the two who developed clear symptoms of plague the same evening, did not benefit by the operation. [illegible].

This completed the first information gathered with regard to five of the six questions enumerated above No answer could be given as to the final duration of the effect of inoculation, except that the operation appeared to be useful in a localised already existing epidemic, extending over seven days

The Experiment in the Umerkadi Common Jail, Bombay

In the next case the strictness of the conditions of the last experiment was enhanced further This was in the second Bombay jail, the Umerkadi Common Jail.

The plague broke out there at the end of December, 1897, and by the 1st of January three prisoners were attacked and all of them subsequently died.

In the interval between the operations in the two jails, some 8000 people in the free population of Bombay had already availed themselves of the inoculation

This time the whole of the prisoners, numbering 401, appeared willing to undergo the preventive treatment. In view of the novelty of the operation, however, and of our responsibilities before the government and the public, and the necessity of demonstrating clearly the effect of inoculation, the prisoners were not allowed to undergo the treatment in a body, and it was resolved that only one half of them should be permitted to do so.

The manner in which that half was selected guaranteed the elimination of all possible errors usually inherent to observations on human communities.

The population of a jail in India is gathered into several groups, the largest being the ordinary convicts divided into simple prisoners and hard labour convicts; then there is a group of civil prisoners, debtors, a group of under-trial prisoners, of convict warders, of cooks, bakers, men employed in the infirmary, &c ; and a separate group of female prisoners.

On the morning of the 1st of January, 1898, in the presence of Major Collie, I.M.S., and Dr. Leon, medical officers, Mr. Mackenzie, the superintendent, and of all the officials of the jail, the above groups were brought one after the other into the jail-yard, and asked to seat themselves in rows, and after all were seated, every second man without further distinction was inoculated, excepting two of them, who did not volunteer for the treatment

From this moment the even numbers, the inoculated, were left to live with the uninoculated, under conditions identical with those under which they were living before They had the same food and drink, the same hours of work and of rest, shared with them the same yards and buildings, &c

In this case fatal attacks continued to occur in the jail for thirty days, during which time an almost equal number of prisoners, inoculated and uninoculated, were discharged from jail, and thus excluded from further observation

The average daily strength of the uninoculated who remained in the jail up to the end exposed to the plague was 127, and of the inoculated, 147.

In the smaller uninoculated number, ten cases of plague occurred, six of them proving fatal; while the larger inoculated number produced three cases, of whom all recovered.

In these three cases, however, in the inoculated, the character of the disease was so much mitigated that the authorities of the Government hospital at Parel, Bombay, where they were sent, hesitated to return them as plague, and the Director-General of the Indian Medical Service, who examined two of them, diagnosed them as mumps. They were returned as cases of plague in order that no possibility of error in favour of the inoculation should be admitted.

The Experiment in the Dharwar Jail.

On the third and last occasion, when the plague broke out in a jail, the authorities did not feel justified in withholding the inoculation from any of the inmates, and all were permitted to be inoculated. This was in Dharwar, at the end of October and the beginning of November, 1898, during the terrible outbreak of plague in that town and district, the news of which must have reached you even here.

Five cases of plague, of which one was imported, and four in old residents of the jail, occurred, all five ending fatally.

The prisoners, then numbering 373, submitted in a body to inoculation, and only one case followed, in a man attacked two days after inoculation, and he recovered, the only one of the six.

The Experiment of Undhera, in a Free Population.

The most carefully planned out and precise demonstration of the working of the prophylactic system among a free population, exposed to a great amount of infection, was that made in the village of Undhera, six miles from Baroda.

The following was the mode of operation adopted :—

A detailed census was made by the authorities of all the inhabitants of the place, and on the 12th February, 1898, when a committee of British and native officers arrived to carry out the inoculation, the people were paraded in the streets, in four wards, family by family.

Major Bannerman, I.M.S., of the Madras Medical Establishment, and myself, accompanied by the Baroda officials, went from one household to another, and, within each, inoculated half the number of the male members, half that of the females, and half that of the children, compensating for odd figures that happened to be in one family by odd figures in another. I, personally, and the officers who were with me, directed special attention to distributing the few sick in the two groups of inoculated and uninoculated as equally as our judgment permitted us to do.

The plague, which had carried off, before inoculation, seventy-nine victims, continued afterwards in this instance for forty-two days and appeared in twenty-eight families, in which the aggregate number of uninoculated was sixty-four, and of inoculated seventy-one.

The total number of attacks in those families was thirty-five, and they were distributed as follows :—

The 64 uninoculated had 27 cases with 26 deaths ,
The 71 inoculated „ 8 „ 3 „ ,

thus showing 89·65 per cent. of deaths fewer in the inoculated members of the families than in the uninoculated.

There were no deaths from other causes in the inoculated of the village, while among the uninoculated there were three deaths attributed to causes other than plague.

The subjoined figures show the number of days which elapsed between the date of inoculation and the occurrence of a death from plague in the families. The first row of figures refers to occurrences in uninoculated members, the second to occurrences in inoculated, while the small figures show the number of deaths which occurred in each group on those days:—

Deaths from plague occurred in uninoculated—
3^2 4^1 5^3 7^2 8^3 –10^3 11^3 12^1 ———— 15^1 16^1 19^1 20^1 21^1 24^1 32^1 and 42^1,
and in inoculated
———— 9^1 ——— 12^1 and 14^1 ————————
days after date of inoculation.

There had elapsed therefore eight days, during which eleven deaths from plague occurred among the uninoculated members of the families, before the first death took place in an inoculated case. The inoculation has again acted, so to say, immediately, or, to use the mode of expression which we have adopted, has exercised its protective effect within the time necessary for the subsidence of the *general* reactionary symptoms produced by the inoculation.

The investigation in this village was carried out by Surgeon-General Harvey, the Director-General of the Indian Medical Service, and a committee of British and native officials. Every member of the family who survived was seen, his particulars verified from the documents, and every detail was confirmed from the registers kept at the time, and from the testimony of the whole of the villagers, who were present throughout the inquiry.

Experiments on a Large Scale. Average of the Results obtained.

I have dwelt so long upon the description of the above experiments not because they were the largest in volume or the most striking which were made, but because they were the most precise of all, and, so far as I am aware, free from any possible loophole of mistake.

I made prolonged and detailed observations in very severely affected communities of Lanowlie, in a population of 700 people, and among the followers of the artillery at Kirkee, numbering at the time 1530. Very complete data were collected by Professor Robert Koch and Professor Gaffky, of the German Government Plague Commission, by Major Lyons, I.M.S., of the Bombay Medical Establishment, and by myself, in the Portuguese colony of Damaon, in a population of 8230 individuals, during a frightful outbreak of plague there, which lasted more than four months, in 1897. A minute investigation extending over four months, was made by me in the Khoja Mussulman community of Bombay, numbering some 12,000 people, where about half of the total number were inoculated under the auspices of His Highness the Aga Khan. A most comprehensive inoculation campaign, and with widely reaching and most satisfactory results, was carried out, under Mr. Cappel, late Collector of Dharwar, by Captain Leumann, I.M.S., Dr. (Miss) Corthorn, Dr. Hornabrook, Dr. Foy, Dr Chenai, and others, in three adjacent small towns of Hubli, Dharwar, and Gadag, where 80,000 people were inoculated. The latter was the most magnificent piece of work done, from the point of view of practical application of the method, and of the testing of its general efficiency.

With the extension of the number of inoculated the exactitude and precision of observation certainly suffer. A number of questions and objections arise with regard to points of detail, which it is not always possible to answer with certainty. Such wholesale observations are, however, required to enable us to judge whether the application of the method as a general measure answers to the expectations formed; whereas the exact extent of the results is only to be gathered from mathematically precise experiments, imitating the conditions of laboratory practice, such as were those which I have detailed above

The difference in the mortality from plague in inoculated and uninoculated sections of communities was estimated to average over 80 per cent., approaching often 90, as was the case in Undhera described above. The lowest proportion ever observed in the experiments which I made personally was 77 9 per cent , this was at Kukee.

Effect on the Case Mortality.

A very accurate set of data were collected in almost all the larger hospitals where inoculated plague cases were admitted, upon the fatality of the disease in inoculated These were to the effect that the case mortality among them was some 50 per cent lower than among uninoculated plague cases A number of documents on this point has been collected by the Indian Plague Commission and will, I trust, appear in their records

Minimum Duration of the Effect of the Plague Inoculation.

As to the duration of the effect of the plague inoculation, the only statement which can be made for the present is that it lasts at least for the duration of one epidemic, which, on the average, extends over four to six months of the year.

The Government of India have recognised the inoculation certificates, entitling the holder to exemption from plague rules, as being valid for a period of six months; on the understanding that if accurate data are forthcoming as to the effect lasting longer, the holders will be permitted to renew their certificates for another period, without being reinoculated.

The further Problems pursued in the Bombay Plague Research Laboratory.

The task which lies now before the Plague Laboratory in connection with prophylactic inoculation comprises the following problems:—

The perfecting of processes for turning out large and uniform quantities of material, and avoiding the variations due to the character of the plague microbe, and to the differences in the composition of the cultivation media;

The further investigation of the different constituents of the plague prophylactic, with a view of intensifying those which produce definite and beneficial results;

The possible mitigation of the reactionary symptoms after inoculation, and

The study of the effect of antiseptics used for preserving the prophylactic,

while the most important general problems concerning plague relate to the study of the curative treatment and to the life-history of the plague microbes in nature.

The Typhoid Inoculation.

The inoculations against cholera and plague, which are the outcome of the work of Jenner, Pasteur, and Koch, and their admirable succession of pupils, contain in their turn a promise of development and success, which, I trust, will be only enhanced at every subsequent step, and which, it seems to me, already warrants the application of the same kind of effort to other diseases and epidemics

In this order of ideas permit me to enter a plea in favour of a new inoculation campaign, which has been inaugurated already, and which I hope will be carried out successfully, for the benefit of a large number of soldiers of this country residing in India, and of white men in general in all tropical countries.

The problem of typhoid inoculation has quite a special interest for Europeans, as much as cholera has for the natives of India. Typhoid proved to be a more difficult disease to eradicate from military cantonments than cholera. It is possible that the explanation of this lies in what is already known of the character of the microbes of these diseases.

The typhoid bacillus when subjected to different chemical and physical agents, such as acids or antiseptics, or a high temperature, or desiccation, or the admixture of other microbes, appears far more resistant than the cholera microbe.

Such a character would ensure for the typhoid bacillus an existence in more varied media, under more various climates, and a greater independence from seasonal or local changes, than is the case with the cholera microbe.

Outside the endemic area cholera remains in one and the same place but for a few weeks, and in any given part of a town often for a few days only. It is rare that it visits the same barrack more than once in five, sometimes ten years, and when it occurs, a temporary evacuation of the place puts a stop to the disease

The typhoid virus, on the contrary, sticks to an infected locality for years, and causes a continuous incidence of the disease for which occasionally nothing short of a complete desertion of the station is effective.

At the same time, while the cholera infection seems to be almost exclusively confined to the water supply, in typhoid the improvement of the water appears to leave intact a large number of other sources of danger which up to the present have escaped detection.

While thus differing in their life-history in nature, the bacilli of cholera and typhoid present important common features in the manner in which they behave in the human and animal body.

The chief centre of infection in both instances is the intestinal canal, the circulatory system remaining free from invasion When inoculated into animals, both microbes admit of the same kind of transformation by passages from animal to animal; and against both immunity can be created in laboratory animals by the same preparation of virus as used in the inoculations for cholera, while, when examining the tissues of immunised animals, the same modifications are detected in them as those observed after the anti-cholera inoculation.

These considerations have led us to expect from the typhoid inoculation in man a similar protective effect to that observed in the inoculation against cholera, and seeing that the period of life during which the newcomers to India remain susceptible to typhoid extends only over a few years, it would seem that the application of the system, when properly organised, is likely to prove of a very high practical value

Inoculation and General Sanitary Measures.

The anti-cholera inoculation, the inoculation against plague, and that against typhoid thus came to put themselves on the same line as vaccination, and represent attempts at dealing with epidemics on a plan differing from measures of general sanitation. During the last few years the question has, therefore, been frequently debated as to the relation in which the two stand to each other

It is scarcely necessary to say that inoculation cannot be substituted for a good water supply, the draining, cleansing, or improvements in the building of cities, or for the admission of a larger amount of light and air into over-crowded localities, for all those measures to which the nations owe the marked improvement in health which has taken place during the present century.

Only, injustice would be done to the sanitarian by calling him in when a patient lies already on his sick-bed, or when an epidemic actually breaks out in a community, and by asking him to stay the sickness, or the epidemic, to improve the health of the population, so to say, while you wait.

To be dealt with, epidemics, like individual diseases, require specifics, promptly administrable remedies, and measures of general sanitation can be no more advised for arresting a sharp outbreak of cholera or plague than an individual patient be directed to build for himself a new house, or to dry up the marshy lands or to cut down the jungle round his habitation when he requires a dose of quinine to arrest an attack of ague

The part of vaccination and of preventive inoculation in combating epidemics stands in the same relation to general sanitary measures as therapeutics and the art of the healing physician stand to domestic hygiene and sanitation It is certain that neither of these can ever be substituted for the other.

Inoculation and the Segregation-Disinfection Method

A comparison of another kind now very actively discussed is that between the methods of combating epidemics by separation of sick and healthy, and disinfection, on the one hand, and by preventive inoculation of the people, on the other.

From this point of view the following distinction between infectious diseases is to be made —

When we take some affected tissue from a leper, or a pustule from a small-pox patient, or virulent saliva from a rabid animal, or some syphilitic matter, and throw it into milk, broth, or any organic substance such as is to be found in the ordinary surroundings of men, it produces no modification in the medium, and in the course of time loses its infective properties and dies out. When, on the other hand, we repeat the experiment with cholera, or plague, or typhoid products, instead of dying out, the contagion begins to grow and multiply, spreads in the medium and soon transforms the whole of it into one mass of infectious matter.

It is evident that such a distinction—the strictly parasitic nature of one microbe and the capacity of the other to lead both a parasitic and saprophytic life—must influence most directly the ways in which these diseases spread and assume epidemic forms, and also the measures which are likely to be effective in combating them.

In the first instance the infection must remain confined entirely, or almost so, to the body of the patient, and the disease can be propagated only directly from individual to individual, or by means of their immediate belongings. It is the inability of a virus to grow in lifeless nature that communicates to that disease a strictly contagious character.

In the second case, provided the surrounding conditions be favourable to it, the virus will spread widely around the original focus, and the sources from which infection reaches fresh individuals will grow in number rapidly

From the point of view of preventive measures, therefore, in diseases like rabies, or syphilis, or small-pox, or leprosy, where infection is to be found in the patient alone, precautions of isolation taken with regard to the sick and their immediate surroundings, must affect directly the prevalence and propagation of the disease, whereas in typhoid, cholera, or plague, where the patient is only one, and proportionately a limited, source of danger, his isolation, and the destruction of his belongings, leaves unaffected the vast cultivations of infection which are going on in nature besides. Measures taken for circumscribing the prevalence of an epidemic by isolating and destroying the foci of infection are less likely to succeed in this category of diseases, attempts at eradicating an epidemic or at protecting individuals by ways which appear effective in merely contagious diseases will be in this case easily eluded, and the necessity of personal protection by means of a prophylactic treatment will soon be urgently felt and acknowledged.

Conclusion. The Officers who assisted in the Bacteriological Investigation in India.

My Lord and Gentlemen—Permit me, before I leave this place, to pay a tribute of gratitude for assistance and co-operation in the investigation work in India to the Officers of the Indian and Bombay Governments, to the Director-General of the Indian Medical Service, to Lieut.-Colonel Owen, Major Bannerman, Major Lyons, Major Herbert, Captain Thorold, Captain Hare, Captain James, Captain Vaughan, Captain Maynard, Captain Earle, Captain Stevens, Captain Green, Captain Clarkson, Captain Milne, Captain Leumann, of the Indian Medical Service, to Dr. (Miss) Corthorn, Dr. Gibson, Dr. Marsh, Dr. Balfour Stewart, Dr Ransome, as well as to Dr W. J. Simpson, Dr. Powell, Dr Mayr, Dr. Surveyor, Dr. Paymaster, Mr. E. H. Hankin, the distinguished bacteriologist of the North-West Provinces of India, to His Highness the Aga Khan, and to a number of other European as well as Indian gentlemen, happily far too numerous to permit of my mentioning them all.

June 15, 1899

The LORD LISTER, F R C S., D.C L, President, in the Chair.

Professor E. W. Brown (elected 1898) was admitted into the Society.

Professor W. F. Barrett, Dr. Charles Booth, Professor A. C. Haddon, Mr. Richard Threlfall, and Mr. A. E Tutton were admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them

A communication on the Echelon-grating Spectroscope was made by Professor A. A Michelson, of the University of Chicago.

The following Papers were read —

I. "A Comparison of Platinum and Gas Thermometers, including a Determination of the Boiling Point of Sulphur on the Nitrogen Scale an Account of Experiments made in the Laboratory of the Bureau International des Poids et Mesures, at Sèvres." By Dr P CHAPPUIS and Dr. J. A. HARKER. Communicated by the Kew Observatory Committee.

II. "A Preliminary Note on the Morphology and Distribution of the Organism found in the Tsetse Fly Disease." By H. G. PLIMMER and J. ROSE BRADFORD, F.R.S.

III. "The Colour Sensations in Terms of Luminosity." By Captain ABNEY, C B., R.E., F.R S.

IV. "On the Orientation of Greek Temples, being the Results of some Observations taken in Greece and Sicily, in May, 1898." By F. C. PENROSE, F.R.S.

V. "The Absorption of Rontgen Rays by Aqueous Solutions of Metallic Salts." By LORD BLYTHSWOOD and E. W. MARCHANT. Communicated by LORD KELVIN, F.R.S.

VI. "On the Oxidation of Carbon at ordinary Temperatures by means of Atmospheric Oxygen with the Production of Electrical Energy." By W. E. CASE. Communicated by Sir WILLIAM PREECE, F.R.S.

VII. "The Conductivity of Heat Insulators." By C G LAMB and W. G. WILSON. Communicated by Professor EWING, F.R.S.

VIII "On Simultaneous Partial Differential Equations and Systems of Pfaffians" By A C DIXON. Communicated by Dr. J. W. L. GLAISHER, F R S.

IX "On the Comparative Efficiency as Condensation Nuclei of Positively and Negatively charged Ions." By C. T R WILSON. Communicated by the Meteorological Council.

X. "Data for the Problem of Evolution in Man. II. A First Study of the Inheritance of Longevity and the Selective Death-rate in Man" By Miss MARY BEETON and Professor KARL PEARSON, F R S.

XI "Collimator Magnets and the Determination of the Earth's Horizontal Magnetic Force" By Dr. C CHREE, F.R.S.

XII "On the Resistance to Torsion of certain Forms of Shafting, with special Reference to the Effect of Keyways." By L. N G FILON Communicated by Professor M. J M. HILL, F.R S

XIII. "On the Waters of the Salt Lake of Urmi." By R T. GÜNTHER and J. J MANLEY. Communicated by Sir JOHN MURRAY, K.C B , F R S.

XIV. "On the Application of Fourier's Double Integrals to Optical Problems" By CHARLES GODFREY. Communicated by Professor J. J. THOMSON, F.R.S.

XV. "On Diselectrification produced by Magnetism. Preliminary Note." By C. E. S PHILLIPS. Communicated by Sir WILLIAM CROOKES, F.R.S

XVI. "On the Orbit of the Part of the Leonid Stream which the Earth encountered on 1898, November 15." By Dr. A. A. RAMBAUT Communicated by Dr. G. J STONEY, F.R.S.

XVII. "On the Numerical Computation of the Functions $G_0(x)$, $G_1(x)$, and $J_n(x\sqrt{i})$; and on the Roots of the Equation $K_n(x) = 0$." By W. STEADMAN ALDIS. Communicated by Professor J. J. THOMSON, F.R.S.

XVIII. "Researches in Absolute Mercurial Thermometry." By (the late) S. A. SWORN. Communicated by Professor H. B. DIXON, F.R S

The Society adjourned over the Long Vacation to Thursday, November 16, 1899.

"A Preliminary Note on the Morphology and Distribution of the Organism found in the Tsetse Fly Disease." By H. G. Plimmer and J. Rose Bradford, F.R.S., Professor Superintendent of the Brown Institution Received June 12,—Read June 15, 1899.

(From the Laboratory of the Brown Institution)

These observations are the result of an inquiry entrusted to us by the Tsetse Fly Committee of the Royal Society, at a meeting of the Committee on March 16, 1899.

The material for our investigations was obtained in the first place from a dog and a rat, inoculated with the blood of a dog suffering from the disease, by Mr. H. E. Durham, at Cambridge

The organism found in the Tsetse Fly disease was discovered by Major Bruce, R.A M.C., F R S, and was classed by him as a Trypanosoma. These belong to the order Flagellata, and, according to Butschli, to the sub-group Monadina

We will, in the first place, describe the adult form of the organism, such as is met with most frequently in the blood of a susceptible animal affected with the disease.

A. *Description of the Adult Form of the Trypanosoma.*

In freshly drawn blood examined as a hanging drop, or as a very thin layer in a cell, the adult form of the Trypanosoma can be easily studied. The latter method is the better, as the organism can be better seen and more accurately examined, in the thin, uniform layer of fluid than in the rounded drop. The easiest method of examining the blood in this way is to make, with a red-hot platinum loop and a small piece of paraffin, a thin ring of paraffin on an ordinary glass slide; the drop of blood is placed in the centre of the ring and a cover-glass placed on it, the thin layer of paraffin preventing pressure If it be desired to keep the blood for continuous examination, it should be drawn into a graduated Pasteur pipette, and one-tenth part of a 5 per cent. solution of sodium citrate should be drawn up after it, then the blood and citrate solution should be carefully mixed in the bulb; the tube should then be sealed up, and drops can be taken from it as desired.

Under ordinary conditions of illumination the Trypanosoma, as seen in the blood, appears to consist of a uniform, homogeneous mass of protoplasm, of worm-like form, with at one end a thick, stiff extremity, and at the other a long, wavy flagellum. It is generally in active motion, and this is seen to be caused by the rapid lashing movement of the flagellum, and by the rapid contractions and relaxations of the mass of protoplasm forming the body, and by the movements of an undulating membrane which is attached to one surface of the body, and which appears to undulate synchronously with the contractions of the protoplasmic body. This membrane is, excepting at the free edge, very transparent, and can be seen much better in citrated blood which has been thickened by the addition of a small drop of 1 per cent. gelatine solution, when its contour and attachments can be much better made out, owing to the slower rate of vibration effected by the thickened medium

The general shape of the Trypanosoma, when rendered quiescent by this means but not killed, is that of a long oval, with one end blunt and the other continued into the flagellum, the membrane is then seen to be attached to one side of the body, it begins a little in front of the blunt end of the organism, and is continued at the end into the flagellum.

But with better illumination, such as a very oblique pencil of rays, or, better still, with monochromatic light (green or blue), the protoplasm is seen not to be homogeneous The organism appears then as a highly refractive body, and near the middle, or between it and the flagellate end, is seen a large dark body much more refractive than the rest of the protoplasm, this is the macronucleus Near the thick, stiff end of the body a tiny still more refractive body (with monochromatic light nearly black) is seen, which is the micronucleus. The addition of a drop of 5 per cent. acetic acid makes both of these bodies much more distinct. At the stiff end of the Trypanosoma, in varying relation to the micronucleus, is seen a vacuole There is no suggestion of a mouth or of any organs, but the protoplasm with the most careful illumination appears not to be uniform, which suggests an alveolar structure, as described by Butschli With the ordinary simple stains (hæmatoxylin, fuchsin, methylene-blue, thionin) the differentiation is not much better than can be observed by careful illumination of living unstained organisms, as these stains are with these, and similar organisms, too diffuse to be of any service. Acting on a method which Ehrlich originated in 1889, and which Romanowsky modified in 1891, and which has still been further elaborated by Ziemann in 1898, we have used a mixture of methylene-blue and erythrosin, which has enabled us to follow the different stages of the Trypanosoma with certainty. This method depends on the fact that when a basic and an acid stain are mixed together in certain proportions, a third neutral body is formed, which has a specific colour reaction with chromatin. By the use of this method we have been able to trace the various stages of the organism in the blood and organs of the affected animals, which is not possible with the ordinary stains, these being useless for many of the forms to be presently described. With this method the macronucleus of the Trypanosoma is stained a clear, transparent, crimson lake, the micronucleus a deep red, and the protoplasm a delicate blue; these reactions are constant throughout all the stages of its life-history.

The protoplasm of the adult Trypanosoma does not stain uniformly, as does that of some of the other forms, but there are parts faintly stained and parts unstained which is again in favour of the alveolar structure mentioned above. The vacuole is quite distinct as a clear round space, when the organism is stained by this method.

The macronucleus is generally of an oval or elongated shape, and it may be either uniform in colour, or in the form of fine threads, this latter is seen especially in those forms which show other signs of division The micronucleus is seen as an intensely stained round dot, or as a short rod, this latter form again being seen in those forms which show other signs of approaching division With the highest powers (1 5 apochromatic objective and 18 compensating eyepiece of Zeiss) we have not been able to make out any special structural characters in this body. The flagellum is not stained by this method, but if the preparation has been well fixed, it is easily visible, the vibratile membrane also is unstained, and can be generally better studied in specimens stained by simple stains, preferably thionin

As regards the movements of the organism, in preparations where no pressure is exercised, they can be seen moving either with the flagellum or with the blunt end in front, but we think that the commoner mode of progression is with the flagellum forward

The size and length of the body varies very much with the period of the disease at which the blood is examined, and with the kind of animal. The largest forms we have seen have been in rat's blood, just after death, and the smallest in rabbit's blood, early in the disease.

B. *Distribution of the Trypanosoma.*

1. *In the body of Normal Animals.*

(*a*) *In the Blood.*—We have found the flagellate form in the greatest numbers in the blood of the mouse, towards the end of the disease. In the rat also they occur in great numbers, and in both these animals they can be found in the blood on the fourth or fifth day. In the dog large numbers can be seen in the blood from the sixth day. In the cat they are fewer in number in the same lapse of time than in any of the animals before mentioned.

The rabbit seems to be the most refractory animal of any we have as yet used, and the Trypanosoma are found in the blood in small numbers only, and at very uncertain intervals.

(*b*) *In the Lymphatic Glands.*—In the superficial glands nearest to the point of inoculation the flagellate organism can be found earliest. In the rat the Trypanosoma can be found in the nearest superficial gland in twenty-four hours after inoculation We have not found that generalisation of the organism in the lymphatic glands occurs until nearly the end of the disease, when the organism is present in very large numbers in the blood. In the rabbit, in which the organisms are few or rare in the blood, the glands do not show any marked change, and the Trypanosoma are not readily found in them. Many other forms are found in the glands, to which reference will be made below.

(*c*) *In the Spleen.*—The adult Trypanosoma is found in but small numbers in the spleens of the various animals we have examined; but other forms are found there which will be described later The enlargement of the spleen is *post mortem* the most obvious fact in the morbid anatomy of the disease, it may attain even to four or five times the average volume—this is especially the case in the rat.

(*d*) *In the Bone-marrow*—We have found either very few flagellate organisms or none at all, in the bone-marrow of the various animals we have worked with. The marrow is altered in colour and structure, but there does not seem to be a greater number of Trypanosoma than can be accounted for by the blood in the marrow

In the other organs and parts the number of organisms present depends upon the relative quantity of blood in the part.

2. *In the Body of Spleenless Animals*—As the spleen in the ordinary animals is the organ which is most obviously altered in this disease, we have made a series of inoculations into animals (dog, cat, and rabbit) from which the spleen had been removed a year ago. In the dog, the adult forms of the Trypanosoma are not found so early in the blood of spleenless as in that of ordinary animals (seventh day as compared with fourth day after inoculation). The glands, after death, are much more generally enlarged, and are reddish in colour, and contain many more organisms than in the normal animal Both the blood and glands contain, however, numerous other forms to be described below.

This marked difference in the colour of the glands of spleenless animals is probably due to the removal of the spleen, and the glands consequently taking on some of the splenic functions.

The bone-marrow is much altered, and in it likewise are found a large number of Trypanosoma: both flagellate and what are termed below "amœboid" forms.

In the cat the conditions of experiment were altered, the blood (1 c.c.) from the infected animal being introduced, with every precaution to avoid contamination of the tissues, direct into the jugular vein. In this case the organism appeared in the blood in numbers on the fourth day, and the animal died on the twelfth day. As the Trypanosoma were introduced into the blood stream direct, there was no marked glandular enlargement, but the glands were all reddish in colour, the change in colour being due to the spleenectomy. A few adult organisms were found in the glands and in the bone-marrow.

In the spleenless rabbit a few Trypanosoma have been found in the blood on two occasions, but the animal lived nearly two months, and notwithstanding the failure to detect adult flagellate forms in the blood on numerous occasions, the blood was always infective, and contained numerous forms termed "amœboid" and "plasmodial" below.

C. *Infectivity.*

(*a*) *In Ordinary Animals.*—The blood and organs of an animal dead of the disease lose, before twenty-four hours after death, their infective power. This is apparently due to the rapidity with which decomposition sets in after death, as we have found living Trypanosoma in film preparations, made as described above, as long as five to six days after removal of the blood from the body, and we have also found that large quantities (200 c.c.) of blood removed from the body into a sterile vessel and kept in an atmosphere of oxygen, retain their virulence for at least three days, notwithstanding the fact that the flagellate form cannot be demonstrated.

We have found that the blood of the dog is infective at least two days before any adult Trypanosoma can be seen in the blood, and we have also found that the blood of the spleenless rabbit, in which we have only on two occasions seen any adult forms, is invariably infective. This of course suggests the idea that the organisms must be present in another form, and we have been able, by the use of the method of staining described above, to demonstrate the presence of other forms in the blood and organs, and have shown, by the experiments just mentioned, that the infectivity of the blood, in cases where there are no flagellate forms discoverable, depends in all probability upon the presence of one of the other forms which the Trypanosoma assumes.

Although a differential staining method, such as the one we have used, is necessary for following and demonstrating the various stages of the life-history of the Trypanosoma, still these stages can be seen in unstained living specimens, with very careful illumination. As a matter of fact, our first observation of them was in unstained preparations.

In the blood of the dog, cat, rabbit, rat, and mouse, besides the adult forms as described above, which, as mentioned, are very various in size, there are adult forms undergoing division, both longitudinal, and transverse, to which reference will be made later. Also two organisms are sometimes seen with their micronuclei in close apposition, or fused together, with more or less of their bodies also merged together. Such forms we believe are conjugations. Again, there are other large forms, with or without a flagellum, in which the chromatin of the macronucleus is broken up into a number of tiny granules, not bigger often than the micronucleus. Besides these there are other forms, which we call for convenience here "amœboid" forms, by which term we mean single, small, irregularly shaped forms, with or without a flagellum, but always with a macro- and micro-nucleus These nuclear structures are generally surrounded by a very delicate envelope of protoplasm, of greater or lesser extent, but occasionally forms are seen which seem to consist only of chromatin, with or without a flagellum. Besides these, again, there are other forms which we call, also for convenience, "plasmodial" forms, meaning thereby an aggregation or fusion of two or more amœboid forms. In the blood these plasmodia are not generally very large, but may show evidence of from two to eight separate elements Signs of division are very common ; but in the blood one does not often meet with a plasmodium dividing up into more than four organisms of the adult shape The plasmodial form also retains intact the two nuclear structures—the macro- and micro-nucleus—which we believe divide in the plasmodium, thus increasing its size.

In the spleenless animals the blood may contain no forms but the amœboid and plasmodial, such as is the case in the rabbit, yet this blood is infective; moreover, in the dog, before the adult organism appears in it, the blood is infective, and therein, at this period, these plasmodial forms can be demonstrated In the glands these plasmodial forms are found, but only in quantity in those animals from which the spleen has been removed.

The spleen is the organ which shows these forms in the greatest abundance The whole spleen is crammed in every part with plasmodia, which are wedged in between the splenic cells in every direction : many amœboid forms, and also immature flagellate forms are also seen, but the most striking thing is the enormous quantity and uniform distribution of the plasmodia. The great enlargement of the spleen, which we have found constant in all the animals we have used, is caused by this mass of plasmodia, which we have found in the spleen within forty-eight hours from the time of inoculation.

In the marrow these plasmodial forms are only found, so far as our experience goes, in those animals from which the spleen has been removed. In these cases there are both plasmodial and amœboid forms in the marrow, the latter the more abundant.

The principal differences in the distribution of the plasmodial forms in animals with and without spleens is this. that in the animals with spleens the organ of choice for the plasmodia is the spleen, but they are also found constantly in the blood, and in less quantity in the glands, whereas in animals from which the spleen has been removed the plasmodial forms are plentiful in the blood, the glands, and the bone-marrow.

D. *Life-History of the* Trypanosoma "Brucii."

Besides the forms mentioned above, we have seen in the blood and in the organs divisions of the adult form, both longitudinal and transverse, the former the more frequent; but we think that this direct mode of reproduction is far less common than the indirect by means of conjugation (probably), breaking up of chromatin, production of amœboid forms, with subsequent division of these amœboid forms, and the formation of plasmodia by the aggregation or fusion of the amœboid forms, and these finally giving off flagellate forms, at first small, and gradually increasing up to the normal adult form.

So that we should tentatively summarise the life-history of the Trypanosoma found in Tsetse Fly disease, which we think might properly be called "*Trypanosoma Brucii*," in recognition of the work done in connection with it by its discoverer Major Bruce, F.R.S., as follows:—

1. Reproduction by division, this being of two kinds:—

   (*a*) Longitudinal, the commoner.
   (*b*) Transverse, less frequent.

2. Conjugation, consisting essentially, so far as our observations go, of fusion of the micronuclei of the conjugating organisms.

(*a*) After this we are inclined to place those forms mentioned above, in which the chromatin is broken up, and scattered more or less uniformly through the whole body of the Trypanosoma, since this occurs after conjugation in other organisms not far removed biologically from this one. The next stage in our opinion is the amœboid, we think that the flagellate form becomes amœboid perhaps after conjugation, but also probably apart from this process.

(*b*) Amœboid forms. These are found with and without flagella, of various shapes and sizes, but always possessing a macro- and micronucleus. These forms are constantly seen in the process of division, and sometimes are very irregular in shape, with, in this case, an unequal number of macro- and micro-nuclei, the latter being the more abundant. The amœboid forms then fuse, or aggregate, together to form—

(*c*) The plasmodial forms. Whether these are true plasmodia, or whether they are only aggregations of amœboid forms it is not yet possible to say, but as many related organisms form true plasmodia we are inclined to look upon these masses, provisionally, as true plasmodia. In the spleen these plasmodia reach a large size. From these again are given off—

(*d*) Flagellate forms, which increase in size, and become the ordinary adult form. Small flagellate forms are not infrequently seen in process of separation from the margin of these plasmodial masses.

Besides these forms we have observed frequently, especially in rat's blood after death, the adult forms arranged in clumps. They appear, upon watching them for a considerable time, to get tangled together to form a large writhing mass; then the movements become gradually slower in the centre of the mass, and are only seen at the periphery. At this stage, if the specimen be fixed, the mass appears to be made up of a quantity of macro- and micro-nuclei, as the protoplasm does not stain, except in the organisms at the periphery, *i.e.*, those which have arrived latest. Eventually these, too, become motionless, and the mass becomes an indistinct collection of granular matter, which is not infective, so that we look upon these tangles as a proof of death.

Since these observations were made, there has been published an important paper on the Rat Trypanosoma, by Lydia Rabinowitsch and Walter Kempner in the 'Zeitschrift fur Hygiene,' vol. 30, Part 2. We have been able to confirm many of the observations and statements as to the morphology and reproduction of the Trypanosoma made by these writers. But there is no mention made of the plasmodial stage, or of any reproductive stage elsewhere than in the blood; and the writers recognize only three methods of reproduction, namely, longitudinal and transverse division, and division by segmentation. This segmentation, they consider, arises from *one* organism, and they state that it may divide up into as many as ten to sixteen elements. This segmentation form would seem to correspond to our plasmodial stage, but we have seen much larger masses than those mentioned above, and they do not notice the enormous masses of plasmodia which infiltrate the spleen in every direction, and which can be found also in glands, and marrow. Moreover, their amœboid stage (Kugelform) would precede the segmentation form, and therefore the "Kugelform" should be much larger than the ordinary adult form, but we have observed that as a rule, our amœboid forms are very much smaller than the adult forms, some not being visible with any but the highest magnifying powers; so that we have been unable to accept this form of division by segmentation, except in the form in which we have described it above, *i.e.*, our plasmodial stage.

"The Colour Sensations in Terms of Luminosity." By Captain W. DE W. ABNEY, C.B., D.C.L., F.R.S. Received February 23, —Read June 15, 1899.

(Abstract.)

This paper deals with a determination of the colour sensations based on the Young theory by means of measures of the luminosity of the three different colour components in a mixed light which matches white. At the red end of the spectrum there is only one colour extending to near C, and there is no mixture of other colours which will match it, however selected. At the violet end of the spectrum, from the extreme violet to near G, the same homogeneity of light exists, but it is apparently due to the stimulation of two sensations, a red and a blue sensation, the latter never being felt unmixed with any other. Having ascertained this, it remained to find that place in the spectrum where the blue sensation was to be found unmixed with any other sensation except white. By trial it was found that close to the blue lithium line this was the case, and that a mixture of this colour and pure red sensation gave the violet of the spectrum when the latter was mixed with a certain quantity of white. The red and the blue sensation being located, it remained to find the green sensation. The complementary colour to the red in the spectrum gave a position in which the green and blue sensations were present in the right proportions to make white, and a point nearer the red gave a point in which the red and blue sensations were present in such proportions as found in white, but there was an excess of green sensation. By preliminary trials this point was found. The position in the spectrum of the yellow colour complementary to the violet was also found. The colour of bichromate of potash was matched by using a pure red and the last-named green. To make the match, white had to be added to the bichromate colour. A certain small percentage of white was found to exist in the light transmitted through a bichromate solution with which the match was made, and this percentage and the added white being deducted from the green used, gave the luminosity of the pure green sensation existing in the spectrum colour which matched the bichromate. Knowing the percentage composition in luminosity of the two sensations at this point, the luminosity of the three sensations in white was determined by matching the bichromate colour with the yellow (complementary to the violet) and the pure red colour sensation. From this equation and from the sensation equation of the bichromate colour already found, the composition of the yellow was determined. By matching white with a mixture of the yellow and the

violet, the sensation equation to white was determined. The other colours of the spectrum were then used in forming white, and from their luminosity equations their percentage composition in sensations were calculated. The percentage curves are shown. The results so obtained were applied to various spectrum luminosity curves, and the sensation curves obtained. The areas of these curves were found, and the ordinates of the green and violet curves increased, so that both their areas were respectively equal to that of the red. This gave three new curves in which the sensations to form white were shown by equal ordinates.

A comparison of the points in the spectrum where the curves cut one another, and of those found by the red and green blind as matching white, show that the two sets are identical, as they should be. The curves of Koenig, drawn on the same supposition, are called attention to, and the difference between his and the new determination pointed out.

The red below the red lithium line, as already pointed out, excites but one (the red) sensation, whilst the green sensation is felt in greatest purity at λ 5140, and the blue at λ 4580, as at these points they are mixed only with the sensation of white, the white being of that whiteness which is seen outside the colour fields.

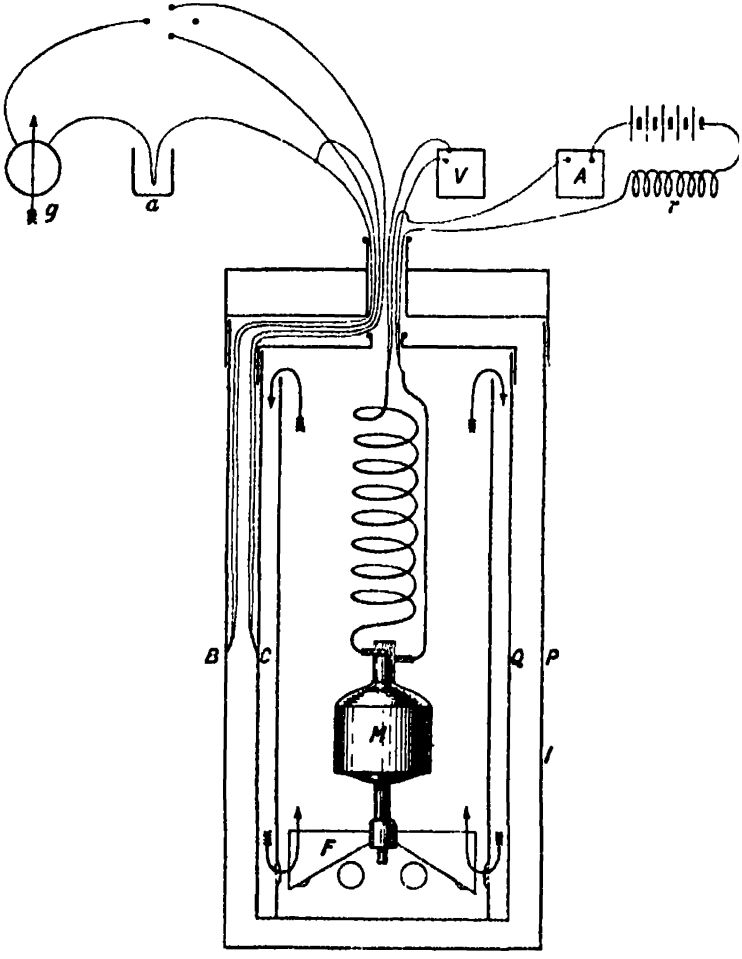
"The Conductivity of Heat Insulators." By C. G. LAMB, M.A., B.Sc., and W. G. WILSON, B.A. Communicated by Professor EWING, F.R.S. Received May 3,—Read June 15, 1899.

The comparative efficiency of materials used as insulators has been the subject of several investigations; the majority of these have been conducted at fairly high temperatures, and it may be questioned whether the results can be applied to the same materials when used as a lagging to protect bodies at low temperatures Moreover the methods adopted do not seem susceptible of any considerable accuracy. The method to be described was devised with the object of using lower temperatures and smaller ranges than had been used in previous experiments, to attain a perfectly steady state of heat transference, and allow of greater accuracy and simplicity in the measurements.

The substances selected for experiment so far have been those which could easily be tested in the dry state, without being made up into cements; they include air, sawdust, charcoal, pine shavings, paper, asbestos, sand, silicate cotton, hair felt, rice husks, and a heat insulator known as "kapok."

The method employed consisted in placing the material under test in the space between two cylindrical copper pots, kept at a definite distance apart by pieces of vulcanised fibre, the inner pot contained a small motor with a fan attached to the axis; a tin-plate cylinder, open at the top and with holes at the bottom, was put inside to direct the currents of air over the inner surface of the inside pot, in the direction of the arrows shown in Fig. 1. Energy was supplied electrically to a heating coil within as well as to the motor: this constituted an internal supply of heat, which maintained the temperature within the pot at any decided upper limit. The motor and heating coil were connected in series, and leads were carried through a small hole in the lid of the pots to measure the current and potential difference, and thus the

Fig. 1.



power expended on internal heating was measured. The outer pot was immersed in a tank kept overflowing from the water main, the lid of the pot being made into a sort of saucer, into which the incoming water ran, thus the outer pot's surface was kept at a uniform and constant temperature. The resulting temperature differences were measured by means of thermo-electric junctions of copper and iron, in a way shortly to be described. The current was allowed to pass steadily into the inner pot, driving the motor and fan, until the inner thermo-electric junction arrived at a steady value, this usually occurred in about three hours or so, when this was the case, the supply of energy by the current was just equal to the heat conducted through the insulator and carried off by the water. Knowing the temperature gradient and the number of watts supplied and the dimensions of the system, we can deduce the specific conductivity of the material

The general arrangement is shown in Fig 1 (P) is the outer pot which stood in the tank, its approximate dimensions were 8 inches in diameter and 16 inches high; (Q) is the inner pot, with one inch clearance between it and the outer one; outside are shown the voltmeter (V), ammeter (A), and adjustable resistance (*r*); inside the pot (Q) are a fixed resistance (R), the motor (M), and fan (F). To a point about the middle of each pot, inside the outer, and outside the inner, are soldered the copper-iron junctions B, C, which are brought outside to a three-way plug and a galvanometer. *a* is a junction placed in a vessel of water at a known temperature; *g* is a galvanometer of the Crompton-D'Arsonval pattern. The junctions used were always made of exactly the same length to keep the total resistance constant, and on calibration were found to give a linear relation between temperature difference and deflection on the galvanometer within the ranges which were to be used, and during each experiment a check calibration at two known temperatures was made by means of a third junction in water at another known temperature. When a steady state was attained, the temperatures of B and C above A were measured by the deflections and calibration tests. The reason for the above indirect method of measuring the difference of temperature between B and C was to avoid possible leakage currents. The flow of heat per second from the inside pot to the outside one will be given by the expression $H = \lambda c\theta$, where c is the specific conductivity, θ is the temperature difference, λ is a constant depending on the size of the pots, being the area in square centimetres of two plane surfaces, distant one centimetre apart, that would permit the same flux of heat as the actual arrangement employed under the same conditions of heat transference and temperature gradient. The value of λ was calculated from careful measurements of their dimensions, on the assumption that the flow of heat could be taken as radial for the sides, and from the top and bottom of the inner, to the bottom and top of the outer, pot; this leads to the expression

$$2\pi\left(\frac{l_1}{\log_e \frac{r_2}{r_1}} + \frac{r_1^2 + r_2^2}{l_2 - l_1}\right) \quad . \quad .$$

for this quantity, where l_1 and l_2 are the lengths, r_1 and r_2 the radii of the pots. This gives for λ the value 1560 when the lengths are measured centimetres If W denote the rate of supply of energy in watts, the rate of transference of heat in gram-C°-units when the steady state is attained will be $H = 0·239\ W$. Hence, if κ is the specific conductivity, and θ the temperature gradient,

$$H = 1560\,\kappa\theta;$$

therefore

$$\kappa = 0·000153\,\frac{W}{\theta}.$$

Two points had to be settled before the determination could be evaluated:—(1) Whether the temperature was uniform over both copper pots; (2) whether any part of the temperature gradient was due to a sudden drop at the surface of separation. As regards the outer pot, since it is wholly surrounded by water, its temperature must be uniform, and the inside and outside can only differ by the drop through the copper, which is eliminated by putting the junction inside that pot, to test the question as regards the inner pot (and partly as regards the outer one) junctions were placed at various points on the surfaces, as well as at certain other points, as shown in Fig. 2. The result of this experiment is given below.

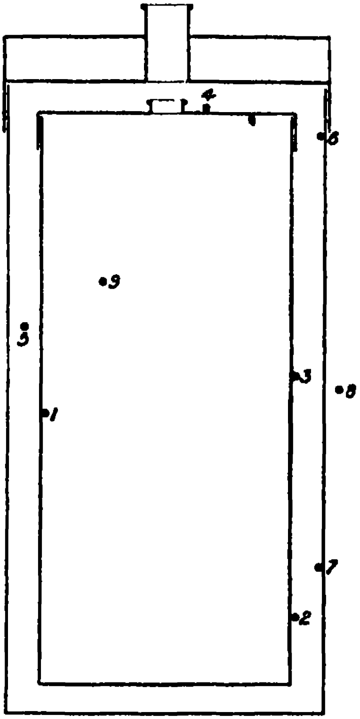
Junction	1	2	3	4	5	6	7	8	9
Temperature relative to 8	25·6	25·6	25·4	25·2	13·8	0·6	0·6	0	28·1

It will be seen that the temperature over the surfaces of the pots was practically uniform, and hence the determination of the gradient could be satisfactorily determined from readings taken with one junction on each pot, as at first described.

The second point was tested as follows:—A third pot was provided, having a clearance of 1 inch with regard to the former larger pot; under similar conditions as to character of insulator and power supplied the temperature gradients in the case of the two combinations of small and medium, and small and large, pots were measured; the ratio was found to be 0·54. The value of λ for the new arrangement of small and large pots was calculated, and found to be 840 in centimetre measure, being thus 0·538 of the standard combination. The close agreement between these values shows that no appreciable drop occurs at the separating surfaces.

These points being settled, the various substances named above were

Fig. 2.



Material.	Temperature differences	Watts supplied.	Watts per 1° C.	*c*.
Air (no baffles)	19·2	25·0	1·30	0·000200
Pine sawdust	19·8	31·7	1·58	0·000242
Pine shavings	19·5	20·8	1·06	0·000162
Brown paper (crumpled up)	20·2	22·0	1·09	0·000167
Hair felt (broken up)	25·8	24·5	0·95	0·000145
Hair felt in two sheets, ½ inch thick each	27·2	18·9	0·69	0·000106
Dry asbestos	14·9	29·0	1·94	0·000297
Charcoal	27·8	27·2	0·98	0·000150
Sand	8·0	39·0	4·86	0·000740
Rice husks	14·0	13·7	0·98	0·000150
Kapok (tight)	23·2	21·8	0·94	0·000144
Kapok (loose)	27·8	22·3	0·80	0·000122
Silicate cotton	24·7	24·5	0·99	0·000151

placed between the pots, and the watts and temperature gradients were determined in the manner described. At least two tests of each material were made, the mean temperature throughout being about 40° centigrade. The results are given in the table (p. 287).

It will be noticed that hair felt was the best insulator that was tested. The insulation in the case of brown paper was practically that of air with subdivided spaces, as the paper occupied relatively a small volume; a comparison of this with insulation by air only will show how great an improvement in air-lagging such a simple expedient will give.

In repeating the experiments with wider ranges and a higher mean temperature, indications were observed tending to show that the conductivity is a function of the temperature. It is hoped to continue the investigation as regards this point, and to extend it to other insulators.

"On the Orientation of Greek Temples, being the Results of some Observations taken in Greece and Sicily, in May, 1898." By F. C. PENROSE, M.A., F.R.S. Received May 5,—Read June 15, 1899.

(Abstract.)

The orientation of the Cabeirion Temple, near Thebes, of which the angle has been disputed (see p. 46 in my paper of 1897), was re-measured with the theodolite in May, 1898, and the previous observations confirmed. An additional example is added from an archaic Temple of Neptune in the Isle of Poros, introducing the employment of the bright zodiacal star Regulus, which I had not before met with.

In Sicily the re-examination of the temples at Girgenti, where, in my former visit, I had relied for azimuth on the sun's shadow and the time, has enabled me to give to the elements some amendments in detail, the only point of consequence being, that the orientation date of the temple named Juno Lacinia is brought within the period of the Hellenic colonisation of that city.

The most interesting point in the paper seems to be, that in the case of two Athenian temples, namely the Theseum and the later Erechtheum—*i.e.*, the temple now partially standing—it is shown that the days of those months on which the sunrise, heralded by the star, illuminated the sanctuary, coincided exactly, on certain years of the Metonic cycle, with the days of the Athenian lunar months on which three important festivals known to be connected with at least one of those temples were held. The years so determined agree remarkably well with the probable dates of the dedication of those temples; and in the case of the first mentioned, the festival, which was named The Thesea, seems to leave little doubt that the traditional name of the temple, which has recently been much disputed, is the correct one.

"On the Comparative Efficiency as Condensation Nuclei of positively and negatively charged Ions." By C. T. R. Wilson, M.A. Communicated by the Meteorological Council. Received May 11,—Read June 15, 1899.

(Abstract.)

The experiments described in this paper were undertaken with the object of throwing some light upon what appeared to be fundamental questions in connection with the electrical effects of precipitation. It was hoped in this way to make some advance towards an understanding of the relation between rain and atmospheric electricity.

It was pointed out by Professor J. J. Thomson* that if positive and negative ions differed in their power of condensing water around them, drops might be formed upon one set of ions only, and separation of positive and negative electricity would then take place by the falling of the drops, the work required for the production of the electric field being due to gravity.

To make this process worthy of consideration as a possible source of atmospheric electricity, it would be necessary to show reason for believing (1) that atmospheric air in the regions in which rain is formed is likely to contain free ions, (2) that positively and negatively charged ions differ in their efficiency as condensation nuclei

It is mainly with the second of these questions that this paper deals. The result of this part of the investigation was to prove that water condenses much more readily on negative than on positive ions. The experiments consisted in measurements of the expansion required to cause condensation in the form of drops in air initially saturated and containing ions, alternately nearly all positive and nearly all negative. The ratio of the final to the initial volume being indicated by v_2/v_1, then, to cause water to condense on negatively charged ions, the supersaturation must reach the limit corresponding to the expansion $v_2/v_1 = 1{\cdot}25$ (approximately a fourfold supersaturation). To make water condense on positively charged ions, the supersaturation must reach the much higher limit, corresponding to the expansion $v_2/v_1 = 1{\cdot}31$ (the supersaturation being then nearly sixfold).

We see, then, that if ions ever act as condensation nuclei in the atmosphere, it must be mainly or solely the negative ones which do so, and thus a preponderance of negative electricity will be carried down by precipitation to the earth's surface.

Incidentally it was proved that the difference between the effects as condensation nuclei of the positively and negatively charged ions is not to be explained by supposing that the charge carried by the nega-

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* 'Phil. Mag.,' December, 1898.

tive ions is, say, twice as great as that carried by the positive ions, for equal numbers of positive and negative ions are produced by the ionisation of the neutral gas.

Attempts were now made to find an answer to the first question suggested above—Is there any evidence that ions are likely to be present under normal conditions in the atmosphere?

Former experiments furnished a certain amount of evidence in favour of an affirmative answer.

When moist dust-free air is allowed to expand suddenly a rain-like condensation always take place if the maximum supersaturation exceeds a certain limit. This limit is identical with that required to make water condense on ions; the identity is in fact so exact as to furnish what is at first sight almost convincing evidence that ordinary moist air is always to a very slight extent ionised. The number of these nuclei is too small to make the absence of sensible electrical conductivity in air under ordinary conditions inconsistent with the view that they are ions.

All attempts, however, to remove these nuclei, by applying a strong electric field such as would have removed ordinary ions almost as fast as they were produced, have failed, even when a differential apparatus was used, such as would have made manifest even a slight diminution in the number of the nuclei by the action of the field. The same is true of the similar nuclei produced by the action of weak ultra-violet light on moist air.

Such nuclei, therefore, in spite of their identity as condensation nuclei with the ions, cannot be regarded as free ions, unless we suppose the ionisation to be developed by the process of producing the supersaturation. This question requires further investigation.

"Data for the Problem of Evolution in Man. II. A First Study of the Inheritance of Longevity and the Selective Death-rate in Man." By Miss MARY BEETON and KARL PEARSON, F.R.S., University College, London. Received May 29,—Read June 15, 1899.

1. According to Wallace and Weismann* the duration of life in any organism is determined by natural selection. An organism lives so long as it is advantageous, not to itself, but to its species, that it should live. But it would be impossible for natural selection to determine the fit duration of life, as it would be impossible for it to fix any other character, unless that character were inherited. Accordingly the hypothesis above referred to supposes that duration of life is an

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* See Weismann, 'On Heredity,' Essays I and II, and especially Professor Poulton's note as to Wallace, p. 23, of first English edition.

inherited character. So far as we are aware, however, neither the above-mentioned naturalists nor any other investigators have published researches bearing on the problem of whether duration of life is or is not inherited. We are accustomed to hear of a particular man that "he comes of a long-lived family," but the quantitative measure of the inheritance of life's duration does not yet seem to have been determined. This absence of investigation appears the more remarkable as a knowledge of the magnitude of inheritance in this respect would, we should conceive, be of primary commercial importance in the consideration of life insurance and of annuities. The biological interest of the problem, as we have already noticed, is very great.

2. It will be well in the first place to point out that the problem is by no means an easy or a straightforward one. The ages at death of even close relatives must be found from records of some kind, or else collected *ab initio.* Now if we take records like the Peerage, Baronetage, Landed Gentry, family histories, and private pedigrees, we find various serious omissions. In the first place the ages of the women are rarely given, pages of the Peerage or Landed Gentry may be examined before a single record is found of the ages of two sisters at death, or even of a mother and a daughter. Further, the census and other returns show how liable we are to find women's ages given erroneously. Family histories and pedigrees suffer in the same way, the pedigrees are mostly taken through the male line, and the women's ages can only be found in rare cases. An exception must be made in the case of the Quaker family histories, such as those of the Backhouse, Whitney, and other families. Here the data are as full for the women as for the men, but naturally the history of a single, even much-branched family, does not provide anything like the material that the Peerage and Landed Gentry do in the case of men. For this reason our first study is confined to inheritance of longevity in the male line We hope eventually to collect enough data for inheritance in the female line, but it will take a longer time to amass, and we fear will scarcely be as homogeneous.

In the second place, the sources to which we have referred omit more or less completely all record of the ages at death of infants and children. The Quaker records give better results than the Peerage, but even here the great bulk of child deaths appears to remain unrecorded. Out of 1000 males born in this country more than 300 die before they are 20 years of age. But when 1000 cases of ages of father and son were taken out of the Landed Gentry, only 31 cases showed the death of a son before 20 years of age. Of 2000 brothers from the Peerage in 1000 pairs, only 21 individuals died before 20 years of age. In the Quaker histories we found about 16 per cent. of deaths before 20 years of age. Clearly such early deaths are not represented in anything like their proper proportions. They will have to be found from other sources; possibly by direct inquiry, and the issue of data cards. We were thus compelled to limit this first study to the case when both relatives die at a greater age than 20. In the case of fathers, when we are dealing with the correlation between father's and son's ages at death, this is practically no limitation at all, as no father dying under 20 years of age was met with. In the case of the offspring, however, the limitation cuts off the distribution somewhat abruptly with a finite ordinate at 20—25, five years being our unit of grouping.

3. Now duration of life is a very different character to eye-colour, or to some extent to the size of organs in adult life. Eye-colour is fairly well determined, it may change with old age slightly, but it cannot transform itself from light blue to brown. Again, nourishment and use undoubtedly affect the size of organs, but they are likely to influence father and son, or, at any rate, brother and brother, in much the same way, for they are members of the same family and the same class. On the other hand, death depends not only on inherited constitution but on innumerable chance elements of environment and circumstance The environment both of home and period is much more alike for two brothers than for a father and son; food, sanitation, habits of life, change considerably in a generation, and two brothers have more equal chances of life than a father and son. But even with two brothers, one may live on the family estates and the other ruin his health in Africa or India. Hence, while the non-differential death-rate will not materially alter the correlations between most characters in relatives, it must seriously affect the correlation between the durations of life in father and son, and to a lesser extent between brother and brother. A good stock may be better protected against death than a weak one, but no stock at all can resist certain attacks. Hence if we look upon death as a marksman, p per cent. of his shots are, we may say, sure to be effective whatever they hit, this is the non-differential death-rate, the remaining $100-p$ per cent. of his attacks will only be successful on the weaker stocks. Now the effect of this conception of death's action is that the correlation table for ages at death of any pair of relatives must be looked upon as a mixture of uncorrelated material—deaths due to the non-differential death-rate, and correlated material—deaths due to the differential or selective death-rate. At different periods of life also one of these death-rates may give more material to the table than at another. In the case of fathers and sons we should expect the non-differential death-rate to be more numerous in its contributions than in the case of brother and brother.

Now it has been shown by one of us that when correlated material is mixed with uncorrelated material, the result is approximately to reduce the coefficient of correlation in the ratio of the amount of correlated to the total amount of material.* Hence, if we assume that the actual correlation between constitutional strengths to resist death would be given, at any rate approximately, by the values determined for other characters in a memoir on the Law of Ancestral Heredity,[*] we have clearly a method of to some extent ascertaining the proportion of the selective and non-selective death-rates in man. In the sequel it will be shown that from the age of 20 to the end of life our tables give a correlation between the duration of life of father and son of about 0·12 to 0·14, and between brother and brother of about 0·26. According to the Law of Ancestral Heredity we should expect these quantities to be about 0·3 and 0·4. Hence we conclude that the amounts of correlated material in the two cases are 40 to 50 per cent. and 65 per cent. But if pN be the number of cases in which the death-rate is selective for N individuals, p^2N will be the number of cases for which it is selective when we take pairs of individuals. In other words the selective death-rate in the first case[†] is about 63 to 70 per cent. and in the second about 80 per cent. of the total death-rate. Without laying great stress on the actual numbers just stated, we think that they are sufficiently close to demonstrate that a substantial selective death-rate actually exists at work on mankind, and that with like environment it may amount to as much as four times the non-selective death-rate.[‡] In other words, having demonstrated that duration of life is really inherited, we have thereby demonstrated that natural selection is very sensibly effective among mankind. The natural selection we are here dealing with is not in the first place, of course, a result of any struggle of individual with individual, but of individual with environment and with the defects of personal physique.

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* 'Phil. Trans.,' A, vol. 192, p. 277.

4. In order to show the biological importance of investigating the inheritance of duration of life, we have cited the results obtained for correlation between the ages at death of father and son and brother and brother. But the method by which these results were obtained requires further discussion. We have already seen the need to exclude deaths under 20 years, but even then we have not got in the case of father and son two like groups of material. The father has been more severely selected than the son. He has lived to become a father, and he is strong enough to be the father of a son who lives to be

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[*] 'Roy. Soc. Proc.,' vol. 62, pp. 397 and 400.

[†] This selective death-rate from the data for father and son must be interpreted in the sense indicated above. The drop from 80 to 65, say, per cent. is in itself a measure of the change of environment of the two generations.

[‡] The correlation on which this determination is based might be illusory, if families were reared under very individual environments; the correlation in duration of life of brothers, for example, might then be a result of their individual family environment. But the environment when we take comparatively homogeneous classes like the Peerage or Landed Gentry must be very similar, and we think this source of error, suggested to us by Professor Weldon, while very real has been sufficiently provided against.

20 years of age. Evidence of this selection is to be found in the facts that (1) fathers have a mean age 5 to 7 years greater than that of their sons; (2) the variability of their age at death is very sensibly less than the variability of their sons' age, *i.e.*, as 2·9 to 3·5, or (3) by noticing for example that in our first table 82 sons as against 20 fathers die before 32·5 years of age, and that in our second table some 100 sons as against 20 fathers die before 35 years of age. Clearly the group "son" is a much weaker type than the group "father." As will be shown in a memoir on the effect of selection on correlation, this want of likeness in fathers and sons itself tends to modify the correlation between them.*

While this selection occurs only in the case of fathers and sons and not in the case of brethren, still the general character of the correlation surface is alike in both. It is known that the curve of frequency of death at different ages† is by no means normal. It is probably compound, and only approximates to normality round three score years and ten. It would hardly, however, fulfil a useful purpose to deal only with the correlation of ages of death of relatives both dying under the old age mortality group, even if on the sunny side of 70 we could distinguish old age from middle age mortality. But in dealing with correlation and regression in such cases as this, we must throw entirely on one side any notion of normal surface and curves of error, and go simply to the kernel of the affair.

What we want is the law connecting the mean age at death of one relative when another relative has died at a given age. When the given age of the latter and the mean age of the former are plotted to form a curve, this curve is the regression curve whatever be the form of the frequency surface. The line of closest fit to this curve is the regression line, and Yule's theorem‡ tells us that the slope of this line is found in exactly the same way as if the frequency surface were a normal distribution. The slope of this line has nothing whatever to do with the particular form of surface, and may be found even if we cut off a portion of the surface parallel to one axis, *e.g.*, if we take the regression line for fathers or sons we get the best fitting lines in precisely the same manner whether we take all sons dying from infancy to old age, or only those from 20 years onwards. If, of course, the regression curve is sensibly linear, then the regression line is the true curve of regression. Everything proved in the memoir, "On the Law of Ancestral Heredity"§ holds for such linear regression equally well; we need not suppose normal correlation. Now the reader has only to look at our regression diagrams, in particular at that for brethren, to assure himself that no curve will serve for practical purposes substantially better than a straight line. Now, if σ_x be the standard deviation of the relative whose mean age at death is taken, and σ_y of the relative of a given age at death, and r be the correlation defined by

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* Not of course very largely, still with the values given in the first series of fathers and sons, the correlation would be reduced about 0·86 to 0·9 of its value by the selection of fathers

† 'Phil Trans.,' A, vol. 186, p. 406 and plate 16.

‡ 'Roy. Soc. Proc.,' vol. 60, p. 480.

§ 'Roy. Soc. Proc.,' vol. 62, p. 386.

$$r = \mathrm{S}\{z(x-m_x)(y-m_y)\}/(\mathrm{N}\sigma_x\sigma_y),$$

where z is the frequency of deviations $x-m_x$ and $y-m_y$ from the means m_x and m_y in the total observations N, then the line of closest fit, the regression line, passes through m_x and m_y, and has $r\sigma_x/\sigma_y$ for its slope. All this is independent of any theory of frequency distribution, and the vanishing of r with the correlation simply flows from the fundamental problem that the chance of a combined event is the product of two independent probabilities. Our conclusions in this paper are deduced from the above value of r and from the slope of the regression line, and they involve no further assumption than the approximate linearity of the regression curves. Our appeal to the memoir, "On the Law of Ancestral Heredity" makes also no greater demands.

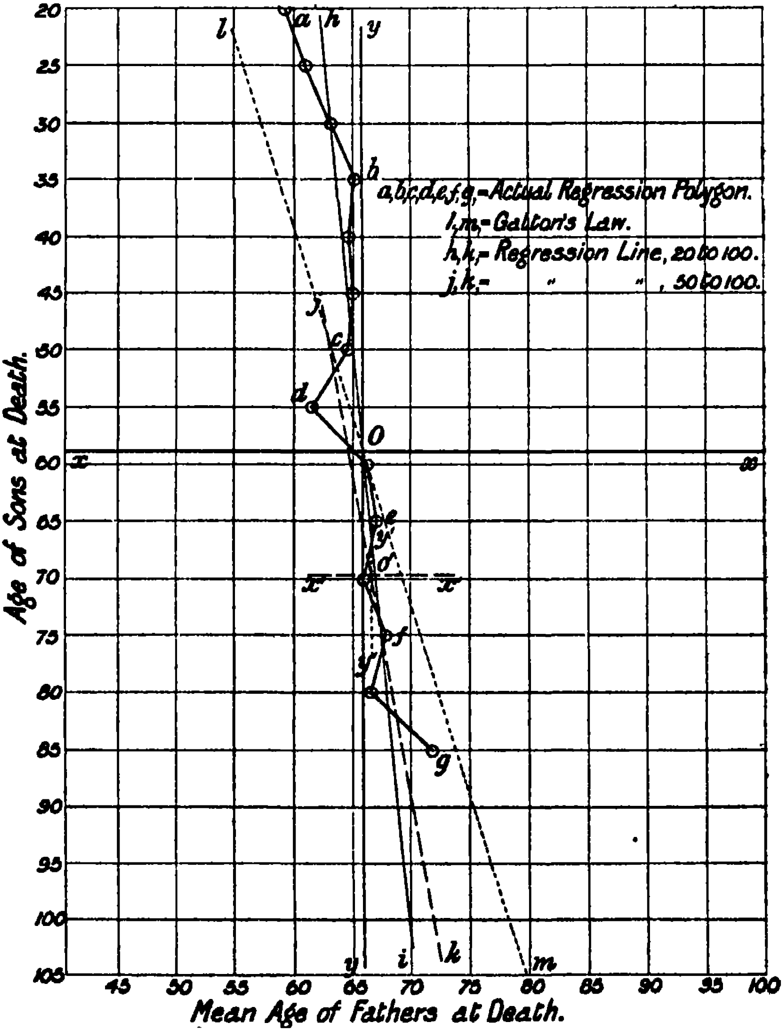
5. We now turn to the material itself. Our data consist of three series, from which all deaths recorded as accidents, an exceedingly small proportion of the whole, were excluded. In excluding these we of course slightly, but very slightly, reduced the non-selective death-rate. In the first series, 1000 cases of the ages of fathers and sons at death, the latter being over 22·5 years of age, were taken from 'Foster's Peerage'; in the second series a 1000 pairs of fathers and sons, the latter dying beyond the age of 20, were taken from 'Burke's Landed Gentry'; and in the third series the ages at death of 1000 pairs of brothers dying beyond the age of 20 were taken from the Peerage.

The first series was obtained by grouping all fathers dying between 22·5 and 27·5, 27·5 and 32·5, &c. We started at 22·5 because this was the earliest recorded death of a father among those extracted from the Peerage, and to have sons dying in the same range they were also started at 22·5 years. In extracting the ages at death, they were taken to the nearest whole year, and consequently in the subsequent grouping we were spared decimals. In the second and third series we originally took all deaths from birth onwards also to the nearest whole year, and then grouped in five-year periods; thus fractions were introduced when a death fell on a five-year division. Subsequently we eliminated the few deaths occurring before 20 years of age.

The aggregate material for the three series is given in Tables I, II, and III; and the means of the arrays of fathers' ages at death for sons dying at a given age, *i.e.*, the regression polygons of fathers' on sons' age at death in figs. 1 and 2; the regression polygon for brethren is given in fig. 3.

In the case of brothers, we have rendered the original distribution, which was nearly symmetrical, absolutely symmetrical, by entering into the table each pair of brothers twice, an individual first appearing as a first brother and then as a second brother. Thus the mean age at death and variability of age at death of both sets of brothers appears the same, and we have a nominal 2000 instead of a 1000 entries. Of course in calculating the probable errors of the constants, 1000 has been taken as the number of observations. We shall now consider these diagrams and tables a little at length.

Fig. 1.—Diagram giving Mean Age of Fathers at Death for Sons dying at a given age. First Series, 1,000 cases.



6. *First Series.*—The means of the arrays of fathers for a given age at death of the son, are shown by the broken line *abcdefg* in fig. 1. The point *a* for the group of sons dying between 17·5 and 22·5 years was put in from a few observations not afterwards included in the table. Beyond the group 82·5 to 87·5 years, there were not sufficient observations to form a reliable mean at all. *yy* gives the mean age of all the 1000 fathers observed, and represents 65·835 years, *xx* gives the mean age of the 1000 sons, and represents 58·775 years. The former may be taken as the mean age at death of all fathers, the latter was only the mean age at the death of sons who live more than 22·5 years. The regression curve is a somewhat broken polygon, but one or two points may be deduced at once from it.

(*a*) It is entirely to the left of *yy* above *xx* and entirely to the right of *yy* below *xx*. Thus there is certainly correlation between the ages at death of father and son. A son dying below the mean age will have on the average a father dying below the mean age, and a son dying above the mean age will have on the average a father dying above the mean age. Graphically we see that correlation must exist. The straight line which best fits the regression polygon is given on the diagram by *hi*. The Law of Ancestral Heredity would give *lm* with a slope of 0·3. It is clear that with a quite sensible regression there is a quite sensible divergence from the law of inheritance, in other words, the death-rate is only in part selective.

Quite similar results are to be observed in fig. 2, there is again a very sensible correlation, but it is sensibly less than that required by the Law of Ancestral Heredity. The lines are lettered the same. Numerically, if M_S, M_F be the mean ages at death of sons and fathers, σ_S, σ_F their standard deviations, r_{SF} their correlation, $R_{SF} = r_{SF}\,\sigma_S/SF$, $R_{FS} = r_{SF}\,\sigma_F/\sigma_S$ the regression coefficients of son on father and father on son, we have—

First Series		Second Series
'Peerage,' Fathers and sons, 25 years and on		'Landed Gentry,' Fathers and sons, 20 years and on
65·835 years	M_F	65·9625 years
58·775 "	M_S	60·9150 "
14·6382 "	σ_F	14·4308 "
17·0872 "	σ_S	17·0986 "
0·1149 ± 0·0210	r_{SF}	0·1418 ± 0·0209
0·0985 ± 0·0182	R_{FS}	0·1196 ± 0·0178
0·1341 ± 0·0367	R_{SF}	0·1682 ± 0·0371

Now these results extracted from very different records are in good accordance. The values of the correlation and regressions are 5 to 7 times the magnitudes of their probable errors, and they agree within the probable error of their differences. The only significant difference is the mean age of deaths of sons in the Landed Gentry, which is some two years higher than in the Peerage. This is the more noteworthy in that we have begun our peerage record at 25 and not 20. Clearly the sons of the Landed Gentry are longer lived. We have undoubtedly correlation, say somewhere about 0·12, sensible and definite in amount, but clearly considerably below the 0·3 required by the law of inheritance.

(*b*) A second point may be noticed by looking at the diagrams (1) and (2), namely, that from about the age of 32·5 to 52·5 the regression line is sensibly vertical, or when the son dies in middle life, the mean age of death of the father is sensibly uncorrelated with it. In other words, we have the remarkable result that the mortality which in a paper on skew variation by one of us,* has been termed that of middle life is largely uninherited. It is during this period of life that the non-selective death-rate is chiefly predominant. After this period the regression curve becomes sensibly steeper, although not fully up to the steepness of the line given by Galton's Law. This is more properly the inheritance of *longevity*. The inheritance of duration of life may not be continuous.

If we seek the best fitting straight line for the regression polygon from 50 years onward we find :—

First Series		Second Series
'Peerage,' 52·5 years of son and on		"Landed Gentry,' 50 years of son and on
66·680 years	M_f	66·878 years
69·686 ,,	M_s	68·960 ,,
14·6734 ,,	σ_f	14·3273 ,,
9·6148 ,,	σ_s	10·4055 ,,
0·1156±0·0232	r_{fs}	0·1125±0·0243
0·1764±0·0380	R_{fs}	0·1549±0·0333

Results such as these are as close as we could expect, and they mark an increase in the steepness of the regression line from about 0·11 to 0·17, an undoubtedly substantial increase of the selective death-rate as we approach old age. The regression line for this old age mortality is marked as *jk* in diagrams (1) and (2), and we see the advance towards the Galtonian value.

(*c*) Below 32·5 years the regression line in figs. 1 and 2, especially the former, seems to indicate increased correlation again, but unfortunately our records do not give enough data to determine its form in a reliable manner

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* 'Phil. Trans.,' A, vol. 186, p. 408, and Plate XVI.

Fig 1 seems to indicate a great approach to the Galtonian value towards youth, and we should not be surprised to find the selective death-rate in youth and infancy even more predominant than in old age This would be the inheritance of the reverse of longevity, of "brachybioty" The regression curve for this portion of life cannot be determined from our present statistics, but we hope to return to it in a second study when more data have been collected * So far as we are able to judge at present the inheritance of the duration of life breaks up into two parts, an inheritance telling its tale in youth and another after middle life It is the former part which seems to us to have most bearing on the fertility and survival of stocks, most individuals having reproduced themselves by 50 years of age It is the latter part only, the true inheritance of longevity, to which it would appear that Weismann and Wallace's arguments apply -†

"For it is evident than when one or more individuals have provided a sufficient number of successors, they themselves, as consumers of nourishment in a constantly increasing degree, are an injury to those successors. Natural selection therefore weeds them out, and in many cases favours such races as die almost immediately after they have left successors"

7 We now turn to the third series, giving the correlation between the ages of death of brothers The data give the following numerical results —

$$M_B = 60\cdot971 \text{ years}$$
$$\sigma_B = 16\cdot8354 \quad ,,$$
$$r_{BB} = 0\cdot2602 \pm 0\cdot0199$$
$$R_{BB} = 0\cdot2602 \pm 0\cdot0216$$

The results here are in good agreement with those for sons in the Landed Gentry, *i e.*, in the second series. The mean age of one of a pair of brothers is slightly greater and the variability of one who has a brother slightly less than in the case of sons But this is exactly what we might expect, considering that "brothers" are a selection from "sons," and a brother is likely to have greater vitality than a son The group sons covers sons of fathers who did not live to have more than *one* son, and who therefore came of any early dying stock, while brothers denotes at least two sons, and therefore on the average some years more life than is necessary for one son.

The values of the coefficients of correlation and regression are some 13 times their probable errors, and we have a substantial correlation,

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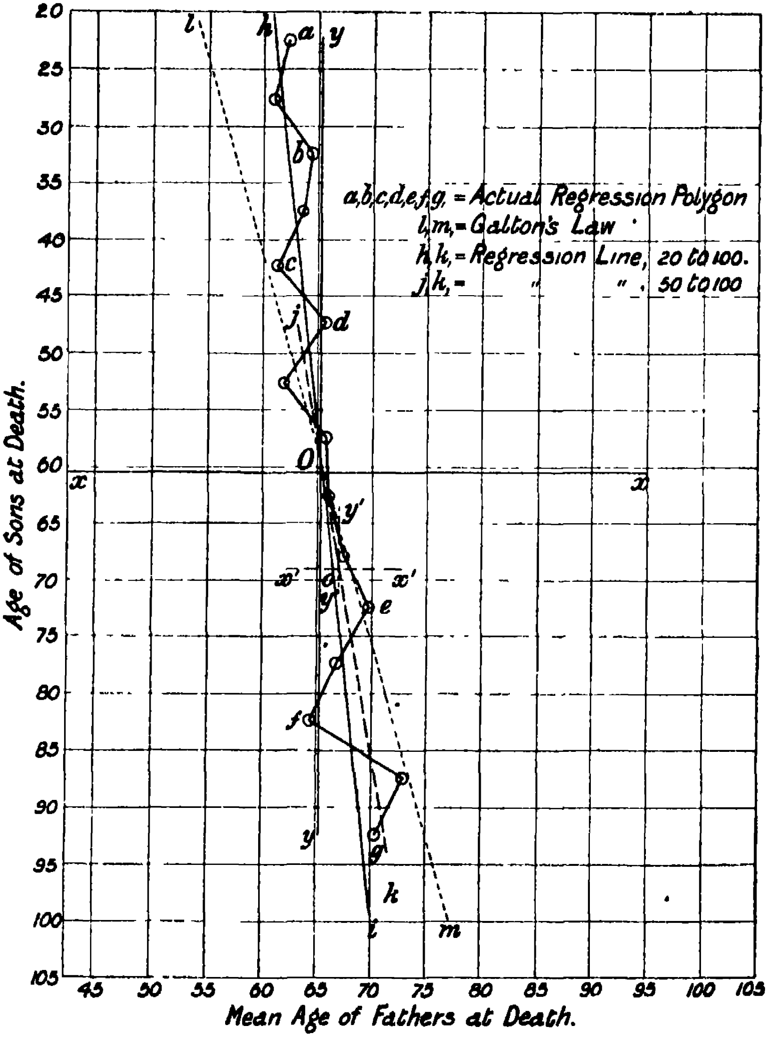
* This collection has already commenced, and we hope shortly to give more definite information on this point

† Wallace, *loc cit.*, *supra*

FIG 2.—Diagram giving Mean Age of Fathers at Death for Sons dying at a given Age. Second Series, 1000 cases.



approaching much closer than in the case of sons to the value demanded (0·4) by the Law of Ancestral Heredity. The diagram shows (i) how substantial is the correlation; (ii) how much more nearly the regression line *kk* given by observation approaches the theoretical line *lm*; and (iii) how very nearly the regression curve is truly linear. The reason of this closer approach to the theoretical value of heredity is owing to the diminution in the non-selective death-rate, the environ ment of brothers during their lives being as a rule much more alike than that of father and son. It must be noted that the predominance of the non-selective death-rate in middle life, so marked in the latter case, no longer appears in the case of brothers. This would suggest that the environments of father and son differ most in middle life and are then much more unlike than those of brothers.

8. We conclude this first study by putting on record formulæ for estimating the age at death of a man, using the theory of multiple correlation as developed in a memoir* by one of the present writers, and taking as basis the second and third series, which seem to us to present the best results.

Let P be the probable age in years at death of a man, F be the age at death of his father, S_1 of his first son, S_2 of his second son, B_1 of his first brother, B_2 of his second brother. Then we have the following cases:—

Prediction of Age at Death. All Deaths after 20 Years.

(*a*) From age of father at death—

$$P = 49{\cdot}8201 + 0{\cdot}1682\,F, \qquad \Sigma = 16{\cdot}9259$$

(*b*) From age of brother at death—

$$P = 45{\cdot}1063 + 0{\cdot}2602\,B_1, \qquad \Sigma = 16{\cdot}2555$$

(*c*) From age of son at death—

$$P = 58{\cdot}6771 + 0{\cdot}1196\,S_1, \qquad \Sigma = 14{\cdot}2850$$

(*d*) From ages of father and brother at death—

$$P = 37{\cdot}6647 + 0{\cdot}12685\,F + 0{\cdot}24502\,B_1, \qquad \Sigma = 16{\cdot}4099$$

(*e*) From ages of father and son at death—

$$P = 48{\cdot}7991 + 0{\cdot}15706\,F + 0{\cdot}11168\,S, \qquad \Sigma = 14{\cdot}1573$$

(*f*) From ages of two brothers at death—

$$P = 35{\cdot}7930 + 0{\cdot}206475\,(B_1 + B_2), \qquad \Sigma = 15{\cdot}9052.$$

(*g*) From ages of two sons at death—

$$P = 54{\cdot}3928 + 0{\cdot}09497\,(S_1 + S_2), \qquad \Sigma = 14{\cdot}1987.$$

(*h*) From ages of brother and son at death—

$$P = 44{\cdot}2601 + 0{\cdot}1046\,S + 0{\cdot}2514\,B, \qquad \Sigma = 13{\cdot}8508.$$

Here Σ is the standard deviation of the array of men for each group. Such formulæ† seem to us to give a quantitative accuracy to much

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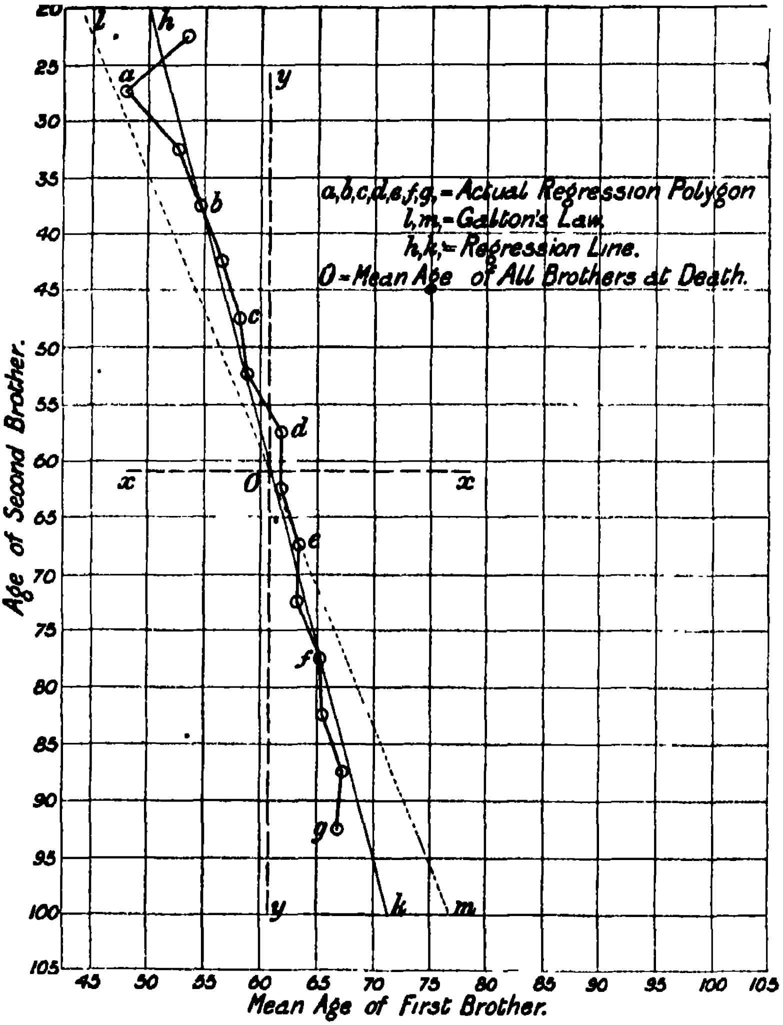
* "Contributions to the Mathematical Theory of Evolution. III. Regression, Heredity, and Panmixia," 'Phil. Trans.,' A, vol. 187, pp. 253–318.

† In obtaining the formulæ for prediction from the age at death of *two* relatives, certain assumptions have had to be made. Thus the correlation of ages of a man and his grandfather and of a man and his uncle at death, being at present unknown, were taken to be half the correlation of father and son. This cannot be far wrong, but the actual values ought to be found. We did not feel justified in assuming the

FIG. 3.—Diagram giving the Mean Age of Man at Death for a Brother dying at a given Age. 1000 cases.



that is allowed rather indefinite weight at present in the actuarial and medical professions. Based on a wider mass of data and a larger series of relationships we cannot but believe they would be of much help to

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variability of grandfathers, which must be less than that of fathers, or their mean age at death, which must be greater than that of fathers, in order to determine the probable age at death of a man from that, say, of his grandfather and father, which would be of much interest. We only wish to draw attention to what we believe to be a new and important field of enquiry, and to indicate the nature of its problems.

the physician and actuary. If their importance were once recognised by the insurance offices, we believe that the necessary data would be readily forthcoming. As illustrations take the following.—

(i) A man's father dies at 40, and his brother at 25. What is the probable reduction in his own life? *Answer* 12 years

(ii) A man has two brothers, who die young at 25 and 29 How much will this shorten his probable duration of life? *Answer* 14 years.

(iii) A man's father died at 40, and his brother, his senior by one year, died at 50, twenty-two years ago A life estate now accrues to the man, whose whereabouts are unknown What is the probability that he is still alive, and should he return and claim the estate, how long is he likely to enjoy it *Answer* The man belongs to an array of *men of mean age* 54·99 *years at death, and standard deviation* 16·41 years. Hence the odds against his living beyond 71 years are 835 to 165, and, accordingly, the odds against the possibility of his return are about 21 to 4 Should he be *alive and return*, he is *as likely as not* to hold it for 6·8 years, and 8·7 years is his expectancy of life, so that the contingency of his being alive and enjoying the estate is worth only *about* 1·4 *years' income of the estate*

Clearly such problems can be extended in a great variety of ways, which might be serviceable in actuarial practice

I.—Correlation Table for the Inheritance of Longevity from Father to Son

First Series

Age of father at death

Age of son at death

20	25	30	35	40	45	50	55	60	65	70	75	80	85	90	95	100	105	Totals.
25	1		1	3	1	1	9	4	3	5	4	3	1	1				37
30	1		3	1	4	1	6	2	6	3	10	5	2	1				45
35			1	2	4	6	4	5	10	6	6	5	7		1			57
40		1	5	2	3	5	6	6	11	7	17	11	2	1				77
45		1	2	2	3	3	5	6	11	11	8	6	4	1				68
50	1	2	2	1	3	7	13	10	4	8	14	11	7	1				84
55	1	2	8	5	5	8	10	7	6	12	9	8	5	2				88
60	1	1		2	2	6	9	6	7	17	13	10	2	2	1			79
65		1	2	5	5	4	11	10	5	18	18	14	10	4				107
70		2	3	1	4	10	9	12	23	9	20	12	10	3	1			119
75	3	1	2	4	3	9	6	10	8	20	23	13	10	6	2			120
80	1		3	1	3	2	6	3	6	9	10	6	8	1				59
85				1	2	1	3	2	6	6	10	8	8	2	1		1	51
90			1		2		2	2		2	2	3						14
95									2		1		1					4
100																		0
105						1												1
Totals	9	11	28	30	44	64	99	85	108	133	165	115	77	25	6	0	1	1000

II.—Correlation Table for the Inheritance of Longevity from Father to Son Second Series.

Age of father at death

Age of son at death	20 to 25	25 to 30	30 to 35	35 to 40	40 to 45	45 to 50	50 to 55	55 to 60	60 to 65	65 to 70	70 to 75	75 to 80	80 to 85	85 to 90	90 to 95	95 to 100	100 to 105	Totals
20 to 25	—	—	—	1·5	3·0	2·0	3·5	5·25	1·25	4·5	6·5	4·0	2·0	—	—	—	—	33·5
25 to 30	—	—	—	2·0	3·0	1·75	4·25	4·25	1·75	4·25	4·75	4·5	1·0	—	—	—	—	31·5
30 to 35	1	—	—	1·5	1·0	1·75	5·75	1·0	2·5	5·25	3·75	4·0	4·25	1·75	—	—	—	33·5
35 to 40	—	0·25	2·25	3·0	1·0	1·5	1·5	7·0	5·0	6·5	1·5	7·0	2·75	2·25	1·0	—	—	42·5
40 to 45	—	0·25	0·75	1·5	4·5	2·5	9·5	5·75	8·25	6·0	5·0	3·0	1·0	1·5	1·0	—	—	50·5
45 to 50	—	2·0	0·5	—	1·0	2·5	9·25	4·5	10·0	3·75	8·0	6·25	6·0	5·75	0·5	—	—	60·0
50 to 55	—	—	—	4·25	7·75	6·5	10·75	7·75	10·75	12·25	7·5	11·5	5·25	0·75	—	—	1	86·0
55 to 60	—	—	—	3·25	3·75	3·25	9·5	10·25	9·5	10·5	11·75	17·0	5·75	4·5	0·5	—	—	89·5
60 to 65	—	1·0	3·0	2·5	2·5	4·75	3·5	10·25	11·5	12·5	12·75	19·75	5·75	3·0	1·25	—	—	94·0
65 to 70	—	2·25	1·25	3·0	6·0	4·0	7·0	12·0	12·5	22·75	15·0	20·25	10·75	8·25	4·75	0·25	—	133·0
70 to 75	—	1·25	1·25	0·5	2·75	4·75	5·0	10·5	16·25	13·75	10·75	23·25	14·25	7·75	3·75	0·75	1	117·5
75 to 80	—	—	2·0	1·5	4·75	11·5	8·25	8·0	20·25	11·5	12·5	12·25	13·0	10·25	2·75	1·0	—	119·5
80 to 85	—	0·5	1·0	3·5	1·0	3·25	5·25	8·0	6·5	7·0	7·5	7·0	2·75	5·75	2·0	—	—	59·0
85 to 90	—	—	—	0·5	2·0	1·5	1·5	0·5	3·0	3·5	4·75	6·5	6·75	6·0	—	1·0	—	37·5
90 to 95	—	—	—	0·5	—	—	1·0	1·0	1·0	1·0	1·0	0·75	2·75	1·0	—	—	—	10·0
95 to 100	—	—	—	—	—	—	—	—	—	—	1·5	—	—	—	—	—	—	1·5
100 to 105	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1·0	—	—	1·0
Totals	1	7·5	12·0	29·0	44·0	51·5	85·5	96·0	120·0	125·0	117·5	147·0	84·0	58·5	18·5	3·0	2	1000·0

III.—Correlation Table for the Inheritance of Longevity in Brethren

Symmetrical Table, 1000 Cases as 2000

Age of first brother at death

Age of second brother at death	20 to 25	25 to 30	30 to 35	35 to 40	40 to 45	45 to 50	50 to 55	55 to 60	60 to 65	65 to 70	70 to 75	75 to 80	80 to 85	85 to 90	90 to 95	95 to 100	100 to 105	Totals
20 to 25	—	3·5	3·5	3·0	2·5	3·5	3·5	—	4·5	1·25	3·5	3·0	0·75	0·5	1·0	—	—	34·0
25 to 30	3·5	15·0	5·0	4·5	3·0	3·5	6·0	5·5	3·0	4·75	1·5	5·0	2·75	1·5	—	—	—	64·5
30 to 35	3·5	5·0	11·5	7·75	6·75	6·5	5·75	8·75	4·0	9·0	7·5	6·0	2·5	3·0	0·5	—	—	88·0
35 to 40	3·0	4·5	7·75	7·0	7·0	7·75	11·25	8·25	9·0	12·0	13·0	2·5	3·0	1·0	0·5	—	—	97·5
40 to 45	2·5	3·0	6·75	7·0	10·5	11·25	10·0	7·0	16·0	6·0	14·75	4·75	8·0	1·0	1·0	—	—	109·5
45 to 50	3·5	3·5	6·5	7·75	11·25	20·5	7·75	9·25	12·75	11·25	22·25	15·5	5·5	2·75	—	—	—	140·0
50 to 55	3·5	6·0	5·75	11·25	10·0	7·75	14·0	14·25	17·25	22·25	12·25	12·75	9·0	5·5	0·5	—	—	152·0
55 to 60	—	5·5	8·75	8·25	7·0	9·25	14·25	15·0	19·0	22·5	13·25	25·25	11·5	6·5	1·5	—	—	167·5
60 to 65	4·5	3·0	4·0	9·0	16·0	12·75	17·25	19·0	21·5	32·5	22·25	19·5	17·0	7·75	1·5	—	—	207·5
65 to 70	1·25	4·75	9·0	12·0	6·0	11·25	22·25	22·5	32·5	28·5	31·25	26·25	16·0	13·75	3·75	—	—	241·0
70 to 75	3·5	1·5	7·5	13·0	14·75	22·25	12·25	13·25	22·25	31·25	38·0	27·0	21·75	10·75	1·5	—	—	240·5
75 to 80	3·0	5·0	6·0	2·5	4·75	15·5	12·75	25·25	19·5	26·25	27·0	34·5	19·75	12·0	3·75	—	—	217·5
80 to 85	0·75	2·75	2·5	3·0	8·0	5·5	9·0	11·5	17·0	16·0	21·75	19·75	14·5	5·5	2·0	—	—	139·5
85 to 90	0·5	1·5	3·0	1·0	1·0	2·75	5·5	6·5	7·75	13·75	10·75	12·0	5·5	8·0	2·0	—	—	81·5
90 to 95	1·0	—	0·5	0·5	1·0	—	0·5	1·5	1·5	3·75	1·5	3·75	2·0	2·0	—	—	—	19·5
95 to 100	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
100 to 105	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Totals	34·0	64·5	88·0	97·5	109·5	140·0	152·0	167·5	207·5	241·0	240·5	217·5	139·5	81·5	19·5	—	—	2000·0

"Collimator Magnets and the Determination of the Earth's Horizontal Magnetic Force." By C. CHREE, Sc.D., LL.D., F.R.S., Superintendent of the Kew Observatory. Communicated by the KEW OBSERVATORY COMMITTEE of the Royal Society. Received May 31,—Read June 15, 1899.

(Abstract.)

During the last forty years, there have been examined at Kew Observatory upwards of 100 collimator magnets used in observing the horizontal force and declination.

The "constants" of these magnets—temperature and induction coefficients, and moment of inertia—have been determined at the Observatory, and the tables based on these determinations have served to reduce magnetic observations at a large number of the leading magnetic observatories.

The present paper deals with the data recorded in the Observatory books for the constants specified above, and with other quantities—such as the "permanent" magnetic moment—which are deducible from the records. It determines the mean values of the several quantities for the instruments of the leading English makers, and investigates whether relations do or do not exist between them. It then deduces from the records the probable errors in the values of the several quantities, proceeding on the hypothesis that the methods of determining them are correct. It next examines, from a mathematical standpoint, the accuracy of the formulæ employed in reducing horizontal force observations, and, from a physical standpoint, the possibility of differences between the quantities determined at the Observatory and the quantities actually concerned in horizontal force observations.

The various sources of uncertainty are dealt with, and an attempt is made to ascertain to what extent they may affect the values found for the horizontal force.

The results of the paper are of too technical a character to admit of their being summarized briefly in an intelligible way.

"The Thermal Expansion of Pure Nickel and Cobalt." By A. E. TUTTON, B.Sc. Communicated by Prof. TILDEN, D.Sc., F.R.S. Received April 18,—Read May 5, 1899.

The following are the numerical experimental data of the eighteen individual determinations of the coefficients of expansion of pure nickel and cobalt, referred to in the abstract previously published (p. 161, *suprà*). Full explanations of the signs employed in the tables will be found in the memoir "On the Thermal Expansion of certain Sulphates."[*]

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[*] 'Phil. Trans.,' A, vol. 192, p. 455.

Thermal Expansion of Nickel.

Experimental Data

L_{t_1}	l.	d.	t_1	t_2	t_3	b_1	b_2	b_3	f_2	Corrn	f'_2	f_3.	Corrn	f'_3
mm.	mm	mm	°	°		mm	mm	mm						
9·800	9·964	0·164	12·4	65·1	119·9	745·5	745·0	744·5	6·40	−0·02	6·38	13·94	−0·03	13·91
			11·4	64·3	119·2	737·4	737·5	738·0	6·44	−0·02	6·42	14·01	−0·03	13·98
			12·1	65·2	118·8	743·0	742·5	742·0	6·47	−0·02	6·45	13·81	−0·03	13·78
10·269	10·425	0·156	8·7	64·7	118·7	745·8	745·9	746·0	7·21	−0·02	7·19	15·04	−0·03	15·01
			8·2	65·1	119·7	748·0	748·1	748·2	7·22	−0·02	7·20	15·18	−0·03	15·15
			9·5	65·2	119·5	750·4	750·6	750·8	7·13	−0·02	7·11	14·96	−0·03	14·93
7·834	7·960	0·126	13·1	68·8	121·6	763·8	764·0	764·2	5·56	−0·01	5·55	11·56	−0·02	11·54
			12·1	66·8	119·7	765·6	765·8	766·0	5·42	−0·01	5·41	11·40	−0·02	11·38
			11·8	66·0	118·6	768·8	768·9	769·0	5·35	−0·01	5·34	11·28	−0·02	11·26

Calculated Expansions.

Diminution of thickness of air-layer		Expansion of tripod screws		Expansion of nickel block.	
$f'_2\lambda/2$	$f'_1\lambda/2$	For $t_2 - t_1$.	For $t_3 - t_1$	$L_{t_2} - L_{t_1}$.	$L_{t_3} - L_{t_1}$
0·0020933	0·0045639	0·0046086	0·0095318	0·0067019	0·0140987
21064	45869	46240	95572	67304	141441
21163	45213	46433	94602	67596	139815
23591	49248	51185	101953	74776	151201
23623	49707	52006	103355	75629	153062
23328	48986	50927	101993	74255	150979
18210	37863	38958	76927	57168	114790
17750	37338	38229	76231	55979	113569
17521	36945	37869	75638	55390	112583

Calculated Linear Coefficients of Expansion

θ	ϕ	L_{t_0}	a	b.
* 0·000 121 54	0·000 000 072 7	9·7985	0·000 012 40	0·000 000 007 4
121 74	72 6	9·7986	12 42	7 4
121 91	69 7	9·7985	12 44	7 1
129 45	72 8	10·2679	12 61	7 1
128 37	79 9	10·2679	12 50	7 8
129 26	72 6	10·2678	12 59	7 1
097 74	59 9	7·8327	12 48	7 6
097 56	60 6	7·8328	12 46	7 7
097 43	61 2	7·8328	12 44	7 8
	Mean values ..	.	0·000 012 48	0·000 000 007 4

The mean coefficient of linear expansion, $a + bt$, of pure nickel, between 0° and $t°$, is thus found to be

$$0{\cdot}000\ 012\ 48 + 0{\cdot}000\ 000\ 007\ 4t, \text{ or } 10^{-8}(1248 + 0{\cdot}74t)$$

The true coefficient, α, of linear expansion at $t°$, or the mean coefficient between any two temperatures whose mean is t, is $\alpha = a + 2bt$, that is

$$0{\cdot}000\ 012\ 48 + 0{\cdot}000\ 000\ 014\ 8t, \text{ or } 10^{-8}(1248 + 1{\cdot}48t).$$

The order of agreement of the nine individual determinations must be regarded as highly satisfactory, and those for each series of three referring to the same direction particularly so. The slight differences in the value of a for the three directions, possibly due to slight internal strain, fully justify the author in having carried out

Thermal Expansion of Cobalt.

Experimental Data.

L_{t_1}	l	d	t_1	t_2	t_3	b_1	b_2	b_3	f_2	Corr.	f'_2	f_3	Corr.	f'_3
mm	mm	mm	°	°	°	mm	mm	mm						
12·976	13·144	0·168	12·6	65·3	120·2	758·5	758·6	758·8	7·85	−0·02	7·83	17·15	−0·04	17·11
			10·8	67·5	119·3	759·4	759·6	759·8	8·22	−0·02	8·20	16·91	−0·04	16·87
			11·9	66·6	120·2	760·2	760·4	760·6	7·81	−0·02	7·79	16·81	−0·04	16·77
11·589	11·696	0·107	10·2	65·1	118·8	763·0	763·3	763·6	6·97	−0·01	6·96	14·89	−0·02	14·87
			8·6	65·3	118·8	764·8	765·1	765·4	7·57	−0·01	7·56	15·43	−0·02	15·41
			5·9	65·4	119·5	768·7	768·8	768·9	8·00	−0·01	7·99	16·00	−0·02	15·98
8·599	8·679	0·080	8·8	66·0	118·0	761·2	760·7	760·2	5·75	−0·01	5·74	11·42	−0·02	11·40
			6·4	65·3	118·2	754·9	754·5	754·0	5·64	−0·01	5·63	11·22	−0·02	11·20
			6·8	65·5	118·2	748·0	747·5	747·0	5·70	−0·01	5·69	11·23	−0·02	11·21

determinations for all the three directions; the mean, however, can be regarded with the fullest confidence as expressing the true coefficient at 0°. The agreement of the values for the constant *b* is really remarkable, considering the extreme smallness of the constant, and is to be attributed to the perfection of the polished surfaces of the nickel block; the mean undoubtedly expresses the true semi-increment per degree of temperature.

Calculated Expansions

Diminution of thickness of air-layer		Expansion of tripod screws		Expansion of cobalt block	
$f'_2\lambda/2$	$f'_1\lambda/2$	For t_2-t_1.	For t_3-t_1	$L_{t_2}-L_{t_1}$	$L_{t_3}-L_{t_1}$
0·0025690	0·0056138	0·0060802	0·0125913	0·0086492	0·0182051
26904	55352	65423	126876	92327	182228
25559	55022	63120	126710	88679	181732
22836	48789	56324	112972	79160	161761
24804	50560	58150	114269	82954	164829
26216	52431	60980	118065	87196	170496
18833	37403	43540	84246	62373	121649
18472	36748	44799	86203	63271	122951
18669	36781	44653	85905	63322	122686

Calculated Linear Coefficients of Expansion

θ	ϕ	L_0	a	b
0·000 156 93	0·000 000 092 3	12·9710	0·000 012 10	0·000 000 007 1
155 10	98 9	12·9743	11 95	7 6
153 79	106 1	12·9742	11 85	8 2
137 52	88 6	11·5876	11 87	7 6
141 21	69 1	11·5878	12 19	6 0
141 89	65 4	11·5882	12 24	5 6
105 65	45 3	8·5981	12 20	5 3
103 95	48 3	8·5983	12 09	5 6
104 77	42 9	8·5983	12 18	5 0
	Mean values ..	..	0·000 012 08	0·000 000 006 4

The mean coefficient of linear expansion, $a+bt$, of pure cobalt, between 0° and $t°$, is thus found to be

$$0{\cdot}000\ 012\ 08+0{\cdot}000\ 000\ 006\ 4t, \quad \text{or} \quad 10^{-8}(1208+0{\cdot}64t).$$

The true coefficient α of linear expansion at $t°$, or the mean coefficient between any two temperatures whose mean is t, is $\alpha = a+2bt$, that is

$$0{\cdot}000\ 012\ 08+0{\cdot}000\ 000\ 012\ 8t, \quad \text{or} \quad 10^{-8}(1208+1{\cdot}28t).$$

The agreement of the individual values is not quite so good as in the case of nickel, owing to the impossibility of obtaining such absolute perfection of the surfaces of the cobalt block as was obtained in the case of the nickel block. In the case of the constant *a* the differences only amount to 3 per cent, and the whole amount of *b* is so minute that one is fortunate in finding the agreement so good. These differences, however, from the nature of their cause, are bound to be on both sides of the truth, and the mean of so large a number as nine is sure to be very near the true value

It will now be interesting to compare these results with those of Fizeau The latter were published in very brief form in the 'Comptes Rendus,' for 1869[*] and also in 'Poggendorff's Annalen,' for the same year.[†] In neither of these publications are any further details given beyond the values of the coefficient of expansion for 40° and the increment per degree, $\Delta\alpha/\Delta\theta$ ($-$ 2*b*), which occur in a table of similar quantities for various metals, together with the information that the specimens of nickel and cobalt employed had been reduced by hydrogen and compressed, and that the range of temperature of the observations was from 10 to 80 The values in question are--

	α 40°	$\Delta\alpha/\Delta\theta$
Nickel . .	0 000 012 79	0·71
Cobalt ...	0 000 012 36	0·80

It will be observed that the values of the coefficient for 40° now presented are higher than those of Fizeau, in the case of nickel the difference is 1307 − 1279 = 0028, and in the case of cobalt 1259 − 1236 = 0023 The author's increments are likewise higher, 148 and 128 against 71 and 80 respectively The fact that the author's increment for nickel is twice as great as Fizeau's might suggest the possibility of a mistake between *b* and 2*b* The author has certainly not made any such mistake, for the mode of calculation employed yields *b* directly, and the values afforded were 74 and 64 respectively. The increment $\Delta\alpha/\Delta\theta$ (Fizeau's θ being the author's *t*) is equally certainly 2*b*. Moreover, the author's value of the increment for aluminium, 2·12, calculated in precisely the same manner, agrees fairly with the value given by Fizeau, 2·29, in the same table in which the values for nickel and cobalt are published. It may be that Fizeau inadvertently gave the value of *b* instead of 2*b* in the particular cases of nickel and cobalt, but it is much more likely that the numbers are correctly given, and that his results were not very concordant with those now given. For Fizeau could certainly not have possessed specimens of nickel and cobalt of the same degree of purity as those supplied to the author by Professor Tilden. The recent discovery of nickel carbonyl has afforded an incomparable means of separating the two metals, and also of isolating nickel from other metallic impurities. Further, the discrepancy between the increment values of the author and of Fizeau for these metals is only of the same order as that between the concordant values of the author and of Benoit, 0·46, on the one hand, and of Fizeau, 0·76, on the other, for the 10 per cent. alloy of platinum-iridium, the value for which Fizeau gives in the same table referred to

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[*] Vol. 68, p 1125.

[†] Vol 138, p. 30,

Taking, therefore, the values published by Fizeau for the increments of nickel and cobalt as correctly representing the results of his experiments, his values of the coefficients at 0°, the constants a, calculated by use of the increment, are as under:—

$$\left.\begin{array}{ll}\text{Nickel} & a = 0\cdot000\ 012\ 51\\ \text{Cobalt} & a = 0\cdot000\ 012\ 04\end{array}\right\}\ \text{Percentage difference } 3\cdot8.$$

At 100° the coefficients would become—

$$\left.\begin{array}{ll}\text{Nickel} & a = 0\cdot000\ 013\ 22\\ \text{Cobalt}\ \ .. & a = 0\cdot000\ 012\ 84\end{array}\right\}\ \text{Percentage difference } 2\cdot9$$

The values thus calculated for the expansion at 0° from Fizeau's data are almost identical with the author's values But the considerable difference between the values and the order of the increments now given and those of Fizeau introduces a different order of progression with rise of temperature According to Fizeau the difference between the coefficients of the two metals is a diminishing one, the percentage difference having fallen from 3·8 at 0° to 2·9 at 100°, whereas the author's determinations indicate that the difference is an accelerating one, rising from 3·2 per cent at 0° to 4·3 at 100°

"On the Waters of the Salt Lake of Urmi."

By R. T. GUNTHER, M.A., and J. J. MANLEY, Daubeny Curator, Magdalen College Communicated by Sir JOHN MURRAY, F.R.S. Received June 8,—Read June 15, 1899.

In June, 1897, a portion of the Government Grant was allotted to one of the authors by the Committee of the Royal Society, for the investigation of the fauna and flora of the great salt lake of Urmi, in Persia, as well as of the relations of that fauna and flora to its environment. The present research was undertaken with the view of placing on record some of the conditions prevailing in the lake at the present day.

The extraordinary changes which the level of the waters of the lake has undergone, and is still undergoing, enhance the importance of periodical examinations of the nature of the waters. The advisability of the preservation of such records was urged upon the Royal Society by its Secretary, 184 years ago. Edmund Halley, in his "Short Account of the Cause of the Saltness of the Ocean, and of the several Lakes that emit no Rivers," expressed himself as follows:—"I recommend it therefore to the *Society*, as opportunity shall offer, to procure the Experiments to be made of the present degree of Saltness of the Ocean, and of as many of these Lakes as can be come at, that they may stand upon Record for the benefit of future Ages."* At the present day there are additional reasons for recording the properties and composition of such salt lakes as are known to be inhabited by life, because Schmankewitsch† and many others of the modern school of *Entwickelungsmechanik* (Morgan, Loeb, Vernon, &c.) have proved that every change in the salinity of the waters is accompanied by definite, rapid, and corresponding changes in the anatomical structure of certain of their halophilous fauna, and especially of the species of *Artemia*, one of which occurs in Lake Urmi.‡

The superficial area covered by Lake Urmi is about 1750 square miles, or about four times that of the Dead Sea. For so large an expanse of water its depth is inconsiderable, the greatest soundings do not exceed 40 feet, and since much of the lake is extremely shallow, its average depth is probably under 20 feet. When viewed from the commanding heights of one of its islands, its waters show that brilliant deep blue colour which is so characteristic of salt lakes, but as seen from a boat, the light which is reflected from the light grey mud at the bottom is green.

The temperature of so small a volume of water, which is at the same time so extended as Lake Urmi, must necessarily vary considerably with the seasonal changes of temperature. During the months of July and August the temperature of the surface waters varied from 27·8° C. to 25·8° C., and the temperature of the bottom water at a depth of about 25 feet was 25° C. The specific gravity of the water, as measured on the spot by an ordinary hydrometer, was 1·11, whether the water was drawn from near the bottom or from the surface of the lake, it may therefore be assumed that the waters of the lake remote from the mouths of the fresh water tributaries were of a fairly uniform density, a result which was probably due to the thorough mixing of the waters produced by the strong south-easterly breezes prevalent at the time.

The total quantity of water available for the more detailed examination was brought home in two glass wine bottles, holding about ¾ litre a-piece. The samples A and B were collected on September 16, 1898, near the base of the Bezau Daghi, on the western shore of the lake, where there is comparatively deep and clear water close in shore. The bottles were carefully corked, and it is, I think, fair to assume that no great changes have occurred in the interval between the bottling of the samples in Persia and their examination in Magdalen College Laboratory, in Oxford, seeing that the discrepancies between the analyses are very small.

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* 'Phil. Trans.,' vol. 29, p. 299, 1715.

† 'Zeitschrift Wiss. Zoologie,' vol. 25, 1875.

‡ Günther, 'Nature,' vol. 58, p. 485.

The examination was both physical and chemical.

PHYSICAL EXAMINATION.

Determination of Specific Gravity.

The specific gravities of the two samples of water (A and B) were determined by Mr. H. N. Dickson, according to the method of Sprengel, with the following mean results —

	A	B.
Specific gravity, at 15° C.	1·11338	1·11389
„ „ 0·3° C. .	1·11891	1·11945
Difference.. . .	0·00553	0 00556

Determination of Refractive Index.

The refractive indices (μ) were determined by means of a hollow quartz prism of 60° 6′ refracting angle, and a large spectrometer* reading to 2″ of arc. The water was at a temperature of 12·2° C. during the readings.

	A	B.
Angle of minimum deviation of D line	25° 50′ 4″	25° 50′ 35″,
whence μ =	1·36110	1·36122

It is believed that similar optical measurements will be found to be applicable to ordinary sea waters and will be found to give a more accurate and a more readily obtained indication of the physical nature of the water than the ordinary specific gravity methods.

Determination of Boiling Point.

It has long been customary to record the boiling points of strongly saline natural waters, but in only too many cases, owing to the lack of description of the conditions of the experiment, the records have only a small value. Many trials have convinced us that the boiling points of brines properly determined under similar conditions yield as reliable, although less minute, information concerning the degree of salinity as specific gravity determinations.

The salt water was boiled in a platinum bottle, to which an inverted condenser containing ice-cold water was attached, in order to prevent loss of water vapour and consequent concentration. The temperature was measured by a form of platinum resistance thermometer, which reads to 0·01° C. Three readings were always taken. Firstly, the temperature of the steam from ordinary boiling water in a steam jacket and under atmospheric pressure, secondly, the temperature of the boiling salt water; and finally, the temperature of steam once more. If the first and last readings were identical, it was considered that the conditions of the experiment had remained constant. As a matter of practice it was found that when once the boiling point of the salt water had been reached, the water continued to boil at that temperature for any length of time, so long as the pressure remained constant.

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* The spectrometer, which was constructed for Dr. Bedson, and the prism employed are the property of the Royal Society.

	A.	B.
Boiling point under normal pressure . .	103 84° C.	103·88° C.

It will thus be seen that the three results of the physical examination of the two samples A and B are all mutually confirmatory, in so far that they indicate that sample B had become a little more concentrated than A during its journey from Persia to Oxford.

CHEMICAL EXAMINATION.

The method adopted was that of Dittmar, as described in the Report on the Composition of Ocean Water.*

For the estimation of the lime and magnesia, 20 c.c. of the water, weighing approximately 22·2 grams, were measured off, and the quantities used in the determination of the potash and total salts were half that amount.

Examination of the Correctness of Dittmar's Factor 0·91 *for "Crude" Lime.*

Forty c.c. of the water were measured off and weighed. In accordance with Dittmar's recommendation, the calcium was precipitated as oxalate, filtered, washed, and finally weighed as oxide. The "crude" oxide obtained amounted to 0·0319 gram: this was then redissolved and again precipitated and weighed as "pure" oxide; the weight was found to be 0·0284 gram. If we multiply the weight of crude lime, 0·0319 gram, by Dittmar's factor 0·91, we obtain 0·0290 gram as the weight of "pure" lime. This amount only differs from that actually found by +0·0006 gram, thus affording confirmatory evidence of the correctness of the factor.

The quantities of pure lime given below were determined by reprecipitation and repurification. The magnesia was precipitated by sodium phosphate instead of by ammonium phosphate as Dittmar recommends, because when the latter reagent was used the magnesia was found to come down very slowly and to adhere inconveniently to the sides of the vessel.

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* 'Challenger Reports,' "Physics and Chemistry," vol. 1.

The soda was determined by the method in which all the bases are converted into normal sulphates, and the weight of the mixed sulphates is diminished by the subtraction of the weights of the potassium, calcium, and magnesium sulphates. The potash was determined by precipitation from the mixed sulphates by chloride of platinum, according to Dittmar's third and final method,* and his observations upon the appreciable solubility of the finely divided platinum by the cold dilute hydrochloric acid employed for washing were confirmed.

From the known amounts of lime, magnesia, and potash (see below) were deduced the following weights of normal sulphates in 100 grams of the mixed sulphates:—

	A Grams	B Grams
Potassium sulphate	0·258	0·259
Calcium sulphate	0·146	0·171
Magnesium sulphate	1·870	1·871
Sodium sulphate (by difference) .	15·547	15·606
Total sulphates	17·821	17·907

The quantities of the principal saline components dissolved in 100 grams of the water of Lake Urmi are—

	A	B.
Lime (CaO)	0·0603	0·0706
Magnesia (MgO)	0·6265	0·6266
Potash (K_2O).	0·1394	0·1402
Soda (Na_2O)	6·788	6·814
Chlorine (Cl) . .	8·496	8·536
Sulphates (SO_3)	0·6205	0·6312
	16·7307	16·8186
Oxygen equivalents of the chlorine to be deducted .	1·9167	1·9258
Total salts in 100 grams of water	14·814	14·893

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* 'Challenger Reports,' *loc. cit.*, p. 16.

Or, recalculated for 100 parts by weight of total salts, we have—

	A.	B.
Chlorine (Cl) . .	57·351	57·315
Sulphates (SO_3) . .	4·189	4·238
Lime (CaO) .	0·407	0·474
Magnesia (MgO)	4·229	4·207
Potash (K_2O) .	0·941	0·941
Soda (Na_2O) . .	45·822	45·753
Deduct [O] per [Cl_2]	12·939	- 12·931
	100·000	99·997

The hypothetical proximate composition of 100 parts of the total salts was calculated, with the following results: —

	A	B i.	B ii.
Sodium chloride	86·332	86·203	86·203
Magnesium chloride .	6·661	6·816	6·816
„ sulphate	4·211	4·150	3·915
Calcium sulphate .	0·988	1·151	1·151
Potassium sulphate	1·741	1·741	1·741
	99·933	100·061	99·826

Result B i was obtained by calculating the magnesium sulphate from the residual sulphate (SO_3). Result B ii from the residual magnesium.

It is a remarkable fact that notwithstanding the occurrence of limestone rocks and pebble beaches in the lake, no combined carbonic acid could be detected in the water; indeed, there would be no base for it to combine with. On the other hand, small quantities of free carbon dioxide were present dissolved in the water, and were estimated by Tornöe's method.*

	A.	B.
Free carbon dioxide in solution	0·028 per cent.	0·017 per cent.

Result A was the mean of two determinations which agreed to within 0·002 per cent., and result B was obtained twice by the use of different standard solutions; the results were identical.

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* Before applying Tornöe's method for the estimation of carbon dioxide to the samples of water A and B, two determinations of combined carbon dioxide in a dilute and standard solution of sodium carbonate were carried out, in order to ascertain the degree of accuracy one might hope for. In the first, the carbon dioxide found only exceeded that known to be present by 0·0003 gram, and in the second by 0·0001 gram.

No iodine or bromine could be detected in the small quantity of water available for examination.

Spectroscopic examination revealed the presence of a trace of barium. The quantity present would have been quite unweighable, and although estimated with the calcium could not have vitiated the results.

The results given under the heading B are regarded as those which most nearly represent the true condition of the lake, and consequently no attempt has been made to strike an average between the two series of results. The A results are given in extenso in order to demonstrate the degree of reliability of the B results, a matter which will be of importance in the future, when, after an interval of some years, another investigation of the water of Lake Urmi is made.

"On the Application of Fourier's Double Integrals to Optical Problems." By CHARLES GODFREY, B.A., Scholar of Trinity College, Isaac Newton Student in the University of Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received June 12,—Read June 15, 1899.

(Abstract.)

The propagation of plane plane-polarised light in the direction of z is governed by the equation $V^2\frac{\delta^2\phi}{\delta z^2} = \frac{\delta^2\phi}{\delta t^2}$, where V is the velocity of light.

The most simple solution of this equation is—

$$\phi = R\cos\left[u\left(t \pm \frac{z}{V}\right)+\psi\right] \quad . \quad . \quad .. \quad (1).$$

This may be interpreted as a train of waves of amplitude R, period $2\pi/u$, and phase ψ, travelling with velocity V. This train of waves is without beginning or end. Most of the results of physical optics have direct application to a disturbance of the above form.

No radiation is found in nature which has the properties of the above function. The fact alone that all natural radiations have beginning and end would suffice to render (1) an inadequate representation.

It is required to solve the problem how to represent any natural radiation faithfully without losing the conveniences connected with the form (1). The problem recalls the familiar process of harmonic analysis. This process is applicable only to periodic functions, whereas such a motion of the æther as constitutes white light is non-periodic. In this connection it has been pointed out by Gouy that Fourier's theorem of double integrals enables us to express a wide class of functions in terms of circular functions. In fact, subject to certain limitations,

$$f(t) = \int_0^\infty (\mathrm{C}\cos ut + \mathrm{S}\sin ut)\,du,$$

where $$\mathrm{C} = \frac{1}{\pi}\int_{-\infty}^{+\infty} f(v)\cos uv\,dv, \qquad \mathrm{S} = \frac{1}{\pi}\int_{-\infty}^{+\infty} f(v)\sin uv\,dv.$$

It is proved in the course of the present paper that this process is always legitimate when $f(t)$ is such a function of time as can occur in a physical problem.

The above theorem reduces $f(t)$ to the limit of a sum of simple circular function of time, the element being $du(\mathrm{C}\cos ut + \mathrm{S}\sin ut)$. If we write this $du\,.\,\mathrm{R}\cos(ut+\psi)$, a simple vibration of amplitude $\mathrm{R}du$, period $2\pi/u$ and phase ψ is suggested. To connect this analysis with the physical analysis of light into a continuous spectrum is tempting. The present essay is an attempt to prove that, in certain very general cases, such a connection exists The proof depends upon the two principles (i) that we can have no cognizance of instants of time, but can observe only the contents of small intervals of time; (ii) that, in spectrum analysis, we do not deal with definite wave-lengths, but rather with small ranges of wave-length.

The fruitfulness of this calculus is illustrated by several applications. The radiation of an incandescent gas is discussed. The trains of waves emitted by molecules are continually being terminated by collisions. It is held that, in dealing with the limiting widths of spectrum lines, this effect must be included in the same investigation with the Doppler effect first pointed out by Lippich and Lord Rayleigh.

Other problems shown to be within the grasp of this method are: the connection between Rontgen rays as explained by Professor Thomson, and ordinary light; and the effect of radiative damping of the molecular vibrations in widening the lines of the spectrum. All these investigations are based upon a theorem for dealing with a radiation composed of a vast aggregate of similar pulses distributed at random. The theorem is due to Lord Rayleigh.

It is usual to examine the theory of dispersion by considering the action of a simple periodic force upon a simple vibrator. Since no light is simply periodic, it is necessary to extend the examination. This is done below. We have also inquired whether fluorescence can be due to natural vibrations of the molecules aroused by the non-periodic quality of light. It is shown that so long as the equation of motion is linear, no such explanation is possible.

"On Diselectrification produced by Magnetism. Preliminary Note." By C. E. S. PHILLIPS. Communicated by Sir WILLIAM CROOKES, F.R.S. Received June 13,—Read June 15, 1899.

The writer has found, that, under certain conditions, an electrified body rapidly loses its charge when in the neighbourhood of a magnetic field. Nor does it, so far, appear essential that there be any relative motion between the lines of magnetic force and the charged body itself.

Preliminary experiments have been made with apparatus consisting of a glass tube six inches long and one inch in diameter, at the centre of which there was cemented upon both the inner and outer surfaces, a strip of tin-foil one inch wide. Suitable connections were then arranged for the purpose of charging either of these metallic layers by means of an electrical machine The pole-pieces of a powerful electro-magnet projected into each end of the glass tube, through an air-tight flange, and in such a manner as to ensure the production of a strong magnetic field at the central portion of the tube. A Sprengel air-pump was used to rarefy the gas within the tube, and, in the first instance, the inner coating of tin-foil was charged positively.

This charge gave rise to the well known free positive and a bound negative charge upon the outer tin-foil coating, the presence of the former being indicated by the divergence of the leaves of an electroscope connected to that coating. While the pressure of the gas within the glass tube was varied over a range of from atmospheric pressure to that represented by 0·2 mm. of mercury, the charge upon the inner coating being either positive or negative showed no appreciable indication of being affected by the turning on or off of the magnet. But at pressures lower than 0·2 mm., and when the inner coating was positive, the sudden collapsing of the electroscope leaves pointed to the removal of the charge through the action of starting or stopping the magnetic flux. Although the effect was more powerful at the moment of making or breaking the magnet circuit, it persisted in a modified degree as long as the magnetic field existed. No such effect was observed when the inner coating was negatively charged, nor was there any action even in the first case when the magnetic pole-pieces, projecting into the tube, were magnetised so as to be either both north or both south.

The leaves of the electroscope were then connected to one of the internal pole-pieces, and it was seen that if sufficient positive electricity were supplied to the inner coating while the magnet was excited, it became rapidly withdrawn, and ultimately resided upon the pole-pieces themselves.

"On the Orbit of the Part of the Leonid Stream which the Earth encountered on the Morning of 1898, November 15." By ARTHUR A. RAMBAUT, M A., D.Sc, Radcliffe Observer. Communicated by G. JOHNSTONE STONEY, M A., D.Sc, F.R.S. Received June 14,—Read June 15, 1899.

For an accurate prediction of the return of the Great Leonid swarm of meteors, it is of the highest importance to determine as accurately as possible the orbit in which each part of the swarm is moving.

As has been pointed out by Drs. Stoney and Downing,* the denser part of the stream, with which we are chiefly concerned, being now drawn out to such a length that it takes more than two years to pass any point of the orbit, it results that the perturbing effect of the several planets will not be the same on different parts of the stream.

Hence it follows that the orbit deduced from the observations made during the great shower of 1866, however reliable they may have been, must not be assumed to represent accurately the track of the meteors which we shall meet when we pass through the node of the orbit next November, even when allowance is made for the perturbations which that part of the stream has suffered in the meanwhile.

But although the determinations of the radiant point of the shower made in 1866 are entitled to a high degree of confidence, owing to the large number of meteors upon which they depend, yet the discrepancies existing between the results of different observers, and the fact that the varying effect of the earth's attraction upon the position of the radiant at different zenith-distances was generally overlooked by the observers at the time, introduce an element of uncertainty into the orbit.

The extent of these discrepancies may be estimated from the "List of Observed Places of the Radiant Point in 1866," as given by Professor A. S. Herschel in the 'Monthly Notices of the Royal Astronomical Society,' vol. 27, p. 19, and the effect of this uncertainty on the resulting orbit may be illustrated by comparing the two sets of elements deduced by Adams and Schiaparelli, respectively, from English observations of the radiant point on that occasion. These are—

	Adams †	Schiaparelli ‡
Period (assumed) . .	P = 33·25 years.	33·25 years.
Mean distance	a = 10·340	10·340
Eccentricity . ..	e = 0·9047	0·9046
Inclination	ι = 16° 46′	17° 44′·5
Longitude of node.. ..	ν = 51° 28′	51° 28′
Longitude of perihelion...	π = 58° 19′	56° 26′

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* 'Roy. Soc. Proc.,' No. 410.

† 'Monthly Notices,' vol. 27; 'The Scientific Papers of John Couch Adams, vol. 1, p. 269.

‡ 'Entwurf einer Astronomischen Theorie der Sternschnuppen,' von J. V. Schiaparelli, p. 57.

The part of the stream through which the earth will pass this year lies between the position of those meteors which the earth encountered last year and that of the meteors of 1866. It is, therefore, of importance to investigate how far the orbit of the meteors observed in November, 1898, agrees with that deduced from the observations of 1866.

Unfortunately the observations obtained in 1898 are fewer than could be desired. Unfavourable weather prevailed almost universally in England. On the Continent meteors were observed at a few stations, but they appear to have been outlying members of the stream, and the denser portion was not encountered until about 19 hrs G.M.T., on the morning of November 15 (civil time). For observations at the time that the earth was passing through the densest part of the stream, we are wholly dependent upon the American observers. The time of maximum display is, owing to the paucity of meteors, somewhat indefinite, and seems to have differed by several hours at different stations. The most reliable accounts, however, agree in placing it between 19 hrs. 30 min. and 22 hrs. G.M.T. on the morning of the 15th, and from a discussion of all the separate determinations, I have been led to adopt 20 hrs. 45 min. as the most probable time of maximum. This agrees exactly with Professor Young's result, who writes, "The maximum was about 3 hrs. 45 min. (Eastern Standard Time) when for about 20 minutes the meteors averaged two or three a minute."*

In Table I are given—the name of the observer, his observatory or station, its longitude and latitude, the Greenwich time, and the R.A. and declination of the radiant point.

In this connection I may remark that in order to contribute to an accurate determination of the orbit, the G.M.T. corresponding to the observation, and the approximate position of the observer are just as essential as the co-ordinates of the radiant itself. In several of the accounts before us I have had to assume that the position of the radiant corresponds to the time given as that of maximum display, or to the middle of the time over which the watch extended.

The longitudes and latitudes of the observers' positions may also in some cases be in error to the extent of several minutes, but they are sufficiently exact to enable me to compute the "zenith-attraction."

In Table I, I have included only those observations which seem to elate to meteors belonging to the dense, or central, part of the stream. hey are all contained in the interval between 18 hrs. 58 min. and hrs. 2 min. G.M.T.

ach of these separate results has to be corrected for the influence he earth's attraction on the paths of the meteors during their oach, and for the influence of the earth's motion on the apparent on of the radiant.

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* 'The Observatory,' December, 1898, p. 459.

Table I.

No.	Name.	Station.	Long.		Lat.		G M.T.		Radiant.				Reference.
									R.A.		Decl.		
			h	m.			h	m					
1	Sawyer	Brighton, Mass. .	4	44	+42°	22′	18	58	148°	45′	+22°	15′	'Astro. Journ,' No 451.
2	Pickering	Harvard	4	45	+42	23	19	45	151	40	+22	17	'Astro Nach,' 3538.
3	Myers.........	Urbana......... .	5	53	+40	6	22	45	150	30	+21	30	'Astroph Journ,' Jan, 1899
4	Barnard	Yerkes	5	54	+42	34	21	33	149	0	+24	0	'Astroph Journ,' Mar, 1899.
5	Young	Princeton .	4	59	+40	21	20	45	151	0	+22	30	'The Observatory,' Dec, 1898.
6	Davis	Columbia	6	9	+38	57	19	15	151	0	+22	0	'Popular Astronomy,' Dec, 1898
7	Smith	Philadelphia.... .	5	1	+39	57	19	25	150	0	+20	0	„ „
8	Culbertson .. .	Hanover	4	49	+43	42	20	30	149	36	+22	12	„ „
9	Payne	Northfield, Minn.	..		..		19	7	148	0	+20	30	„ „
10	„	„ .	6	13	+44	28	20	52	149	36	+20	54	„ „
11	Wilson	„ .	..		..		23	2	149	36	+21	30	„ „
12	Bracket	Claremont, Cal	7	51	+34	5	21	17	150	53	+22	20	„ Jan., 1899.
13	Wilson	Northfield	6	13	+44	28	22	0	151	30	+22	18	„ Dec., 1898.

The radiants Nos. 2 and 13 were determined by the aid of photography.

The former varies with the zenith-distance of the radiant and its elongation from the "apex," or the point of the heavens towards which the earth is moving at the time. It has always the effect of displacing the radiant towards the observer's zenith, and hence has been called by Schiaparelli the "zenith-attraction." The amount of this displacement (η) is given by the expression,

$$\tan \tfrac{1}{2}\eta = \frac{w-u}{w+u} \tan \tfrac{1}{2}z,$$

in which z is the apparent zenith-distance of the radiant, u is the velocity of the meteors relatively to the earth before the influence of the earth's attraction has become sensible, while w is the accelerated velocity with which the meteor encounters the earth.

This displacement, which has been too frequently overlooked by meteor observers, may, as pointed out by Schiaparelli, amount in extreme cases to as much as 25° 38′. In the case of the Leonids, however, it happens that the elongation of the radiant from the "apex" is so small (in the case before us not exceeding 11° at any time) that the effect of the zenith-attraction never amounts to half a degree, its greatest value being 29′.

For computing the value of w we have the expression

$$w^2 = u^2 + 2gr,$$

r being the earth's radius, and g the acceleration of gravity at the surface, or, expressing the velocities, as is convenient, in terms of the mean velocity of the earth in its orbit,

$$w^2 = u^2 + 0{\cdot}141587.$$

In computing the value of $2gr$ in the above expression the Sun's parallax has been taken to be 8″·80, and the ratio of the earth's mass to that of the Sun equal to 1/331,100.

We thus have for computing w

$$w = u + [8{\cdot}84999] \times \frac{1}{u} - [7{\cdot}39895] \times \frac{1}{u^3},$$

the figures in brackets being the logarithms of the coefficients.

For determining u we may with quite sufficient precision adopt Adams's orbit of 1866. Also if U denotes the velocity of the earth at any time expressed in terms of its mean velocity, and R its distance from the Sun, then,

$$U = \sqrt{\frac{2}{R} - 1},$$

or, neglecting the second power of the eccentricity we may, without loss of accuracy, write U = 1/R.

In Table II are given the values of the Sun's longitude (☉), the longitude (l), the right ascension (a) and the declination (d) of the apex, and the orbital velocity of the meteors (v), all of which can be computed from the 'Nautical Almanac' or from Adams's orbit of the meteor stream. These are given for three epochs, viz.—November 14·0d., 14 5d, and 15 0d, from which their values at the time of each observation may be obtained by interpolation All of these quantities are needed in the subsequent reduction of the observations, or for deducing the elements of the orbit.

Table II.

Time.	☉	Log R.	l.	a	d	r
1898, Nov, 14 0	232° 9′·8	0·99514	142° 52′ 8	145° 13′·7	+13° 53′ 8	1·3877
14 5	232 40 0	·99510	143 22·7	145 42 7	44 0	·3878
15 ·0	233 10·3	·99505	143 52 7	146 11 9	34·1	3879

We next compute the quantities u, w, and η, exhibited in Table III.

Table III.

No	u	w.	η
1	2·3779	2 4075	22′
2	·3718	4014	19
3	·3782	·4078	10
4	·3680	·3977	15
5	·3725	4021	15
6	·3748	4014	29
7	·3845	·4140	22
8	·3767	·4063	15
9	3857	4152	29
10	·3821	·4116	20
11	·3799	4094	11
12	·3736	4032	27
13	·3724	·4020	15

Applying the corrections for the earth's attraction, $d\alpha$ and $d\delta$, to the R.A. and declination from the formulæ

$$d\alpha = \eta \sin p \sec \delta ; \; d\delta = -\eta \cos p,$$

p being the parallactic angle, we find the corrected R.A and declination of the radiant, as given in the second and third columns of Table IV. In the next two columns of the same table are found the longitude (L′) and latitude (B′) of the same points, and in the sixth and seventh

Table IV.

No	α′.	δ′.	L′.	B′.	L	B.	Wt.
1	149° 4′	+22° 2′	143° 35′	+ 8° 54′	143° 31′	+15° 22′	0·5
2	151 56	22 6	146 5	9 53	147 54	17 3	1·0
3	150 37	21 22	145 11	8 46	146 12	15 6	0·5
4	149 12	23 51	143 4	10 38	142 31	18 22	0·5
5	151 13	22 21	145 22	9 53	146 36	17 4	0·4
6	151 26	21 43	145 47	9 21	147 23	16 9	0·3
7	150 19	19 47	145 29	7 11	146 50	12 24	0·2
8	149 48	22 2	144 14	9 8	144 37	15 47	0·6
9	148 23	20 11	143 38	6 56	143 36	11 59	0·1
10	149 52	20 41	144 46	7 53	145 32	13 37	0·3
11	149 43	21 21	144 24	8 28	144 50	14 38	0·3
12	151 8	22 7	145 23	9 38	146 37	16 38	0 2
13	151 42	22 7	145 52	9 50	147 26	16 53	0·5

columns are given the longitude (L) and latitude (B), of the true radiant corrected for the effect of the earth's orbital motion.

The quantities L and B define the direction of the tangent to the orbit of the meteors at the point where the earth intersects it, and from the mean of these separate determinations, the position of the earth in its orbit at the time, and an assumption with regard to the period of the meteor stream, the orbit is to be determined.*

The only difficulty lies in deciding on the best mode of combining the various observations, or in laying down a rule for determining the weights. In this part of the work a certain amount of arbitrariness is, I think, unavoidable. From a careful consideration of all the circumstances of each case, as far as they are recorded, the experience or inexperience of the observer in this class of work as far as it is stated, the number of meteors observed, and the size of the area from which the meteors appeared to radiate, I have been led to adopt the weights given in the last column of Table IV, which represents, I think very fairly, the relative value of the individual observations. It will be noticed that I have given the two photographic results (Nos. 2 and 13) an importance out of all proportion to the number of trails photographed, viz., four trails in the case of No. 2, and two trails in the case of No. 13. This is, I think, justified by the superior accuracy of photographic results in this class of observations.

I thus find as the definitive position of the radiant of the Leonid meteors of 1898,

$$145^\circ\ 49' \pm 20'{\cdot}5\,;\quad +16^\circ\ 2' \pm 19'{\cdot}9,$$

corresponding to the epoch November 14·864 (astronomical time).

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* See 'Handwörterbuch der Astronomie,' herausgegeben von Dr. W. Valentiner, vol. 2, Breslau, 1898.

From the researches of the late Professor H. A. Newton, and the results of the investigations of Drs. Stoney and Downing on the perturbations of Adams's orbit, the most probable value of the period of revolution would appear to be at present about 33·49 years, corresponding to mean distance of 10·39.

Any admissible variation in the length of the period, however, makes but a small change in the other elements of the orbit, as is evident from Table V, in which the elements in each column have been computed with the value of the mean distance which is contained in it.

Table V

		I	II.	III.
(Assumed) period	P =	33·25 yrs	33·49 yrs	33·73 yrs.
Mean distance............	a =	10·34	10·39	10·44
Angle of eccentricity	ϕ =	64° 46′	64° 50′	64° 54′
Inclination	i =	16 3	16 3	16 3
Longitude of descending node	ν =	53 2	53 2	53 2
Longitude of perihelion.. . .	π =	58 40	58 40	58 40

If we adopt the value 10·39 for the mean distance, as being on the whole the most probable, we have the orbit in column II representing the result of the observations of 1898, as far as they have been published.

"A Comparison of Platinum and Gas Thermometers, including a Determination of the Boiling Point of Sulphur on the Nitrogen Scale: an Account of Experiments made in the Laboratory of the Bureau International des Poids et Mesures, at Sèvres" By Drs. J. A. HARKER and P. CHAPPUIS. Communicated by the KEW OBSERVATORY COMMITTEE. Received June 8,—Read June 15, 1899.

(Abstract.)

In 1886, Professor Callendar drew attention to the method of measuring temperature, based on the determination of the electrical resistance of a platinum wire. He showed that the method was capable of a very general application, and that the platinum resistance thermometer was an instrument giving consistent and accurate results over a very wide temperature range.

Callendar pointed out that if R_0 denote the resistance of the spiral of a particular platinum thermometer at 0°, and R_1 its resistance at 100°, we may establish for the particular wire a temperature scale,

which we may call *the scale of platinum temperatures*, such that if R be the resistance at any temperature T°, this temperature on the platinum scale will be $\frac{R}{R_1 - R_0} \times 100$ degrees For this quantity Callendar employs the symbol *pt*, its value depending on the sample of platinum chosen.

In order to reduce to the standard scale of temperature the indications of any platinum thermometer, it is necessary to know the law connecting T and *pt*. These are, of course, identical at 0° and 100°, but the determination of the curve expressing the relationship between them is a matter for experiment.

The work of Callendar had established for a particular sample of platinum the relation

$$d = T - pt = \delta\left[\left(\frac{T}{100}\right)^2 - \frac{T}{100}\right]$$

over the range 0° to 600°, T being measured on the constant pressure air scale.

Later experiments by Callendar and Griffiths showed that this relation holds for platinum wires generally, provided they are not very impure. They propose that the value of δ, the constant employed in the formula, should be determined by taking the resistance of the thermometer in the vapour of sulphur. A new determination of this point on the air scale made by them gave 444·53°, as the boiling point under 760 mm. pressure

The present paper is the outcome of the co-operation of the Kew Observatory Committee and the authorities of the International Bureau of Weights and Measures at Sèvres, for the purpose of carrying out a comparison of some platinum thermometers with the recognised international standards.

A new resistance-box, designed for this work, and special platinum thermometers together with the other accessories needed were constructed for the Kew Committee, and after their working had been tested at Kew, were set up at the laboratory at Sèvres in August, 1897. The comparisons executed between these instruments and the standards of the Bureau may be divided into several groups. The first group of experiments covers the range − 23° to 80°, and consists of direct comparisons between each platinum thermometer and the primary mercury standards of the Bureau. Above 80° the mercury thermometers were replaced by a gas-thermometer, constructed for measurements up to high temperatures. The comparisons between 80° and 200° were made in a vertical bath of stirred oil, heated by different liquids boiling under varying pressures. For work above 200° a bath of mixed nitrates of potash and soda was substituted for the oil tank. In this bath comparisons of the two principal platinum thermometers with the gas-thermometer were made up to 460°; and with a third thermometer, which was provided with a porcelain tube, we were able to go up to 590°. Comparisons of the platinum and gas-scales were carried out at over 150 different points, each comparison consisting of either ten or twenty readings of the different instruments.

By the intermediary of the platinum thermometers a determination of the boiling point of sulphur on the nitrogen scale was also made. The mean of three very concordant sets of determinations with the different thermometers gave 445°·27 as the boiling point on the scale of the constant volume nitrogen thermometer, a value differing only 0°·7 from that found by Callendar and Griffiths for the same temperature expressed on the constant pressure air scale

If for the reduction of the platinum temperatures in our comparisons we adopt the parabolic formula, and the value of δ obtained by assuming our new number for the sulphur-point, we find that below 100° the differences between the observed values on the nitrogen scale and those deduced from the platinum thermometer are exceedingly small, and that even at the highest temperatures the differences only amount to a few tenths of a degree.

Full details as to the instruments employed and the methods adopted are given in the paper.

"Agricultural, Botanical, and Chemical Results of Experiments on the Mixed Herbage of Permanent Grass-land, conducted for many Years in succession on the same Land. Part III.—The Chemical Results." By Sir John Bennet Lawes, Bart., D.C.L., Sc.D., F.R.S., and Sir J. Henry Gilbert, LL.D., Sc.D., F.R.S. Received August 10, 1899

(Abstract.)

The experiments were commenced in 1856, and are still in progress, so that the present is the forty-fourth year of their continuance. There are about twenty plots, two of which have been continuously unmanured, and the remainder have respectively received different descriptions or quantities of manure of known composition. A report on the "Agricultural Results" was published in the 'Phil. Trans.,' Part I, 1880; and a second on the "Botanical Results" in the 'Phil Trans.,' Part IV, 1882. The present paper deals with a portion of the "Chemical Results."

In all cases, of both first and second crops, the dry matter and the ash, and in most the nitrogen, have been determined. In selected cases determinations have been made of the amount of nitrogen existing as albuminoids, and in some of the amount of "crude woody fibre," and of crude fatty matter. More than 200 complete ash-analyses have also been executed.

It was found that the chemical composition of the mixed herbage was very directly dependent, not only on the seasons and on the supplies within the soil, but very prominently also on the description of plants encouraged, and on the character of their development; so that it was essential to a proper interpretation of the variations in the chemical composition, to bear in mind the differences in the botanical composition. Hence a summary table was given showing the characteristic differences in the botanical composition under the different conditions as to manuring, the influence of which on the chemical composition it was sought to illustrate.

As the investigation involved the consideration of the chemical composition of the mixed produce of about twenty plots over forty or more seasons, including the discussion of the results of more than 200 complete analyses of the ashes of the separated or the mixed herbage, attention was called to the state of existing knowledge as to the *rôle* or function in vegetation of the individual constituents found in the ashes of plants, and this was seen to be very imperfect. Further, in calculating the percentage composition of the "pure ash," the plan usually adopted was to exclude not only the sand and charcoal, but also the carbonic acid. The authors considered, however, that the presence and the amount of carbonic acid associated with the fixed constituents in plant-ashes was a point of considerable significance, and they entered into some detail as to the methods of determining the carbonic acid in ashes, and as to the results obtained.

In order to throw some light on the connection between the growth of the crops and their mineral composition, results relating to the separated gramineous, the separated leguminous, and the separated "miscellaneous" herbage of the mixed produce, grown without manure and by different manures, were first discussed. To obtain more definite evidence illustrating the connection between character and stage of growth and the composition of the products—especially the ash-composition—results relating to the bean plant, taken at successive periods of growth, and also to the first, second, and third crops of clover, were next considered. Lastly, in further illustration, results as to the nitrogen and the ash-composition of crops of three different natural orders—wheat representing the Gramineæ, Swedish turnips the Cruciferæ, and beans and clover the Leguminoseæ—were given.

The general result was, that there were very characteristic differences in the composition of the ashes of different crops according to the amounts of nitrogen they assimilated. Red clover, for example, yields large amounts of nitrogen over a given area, part of which is due to fixation, but much is certainly taken up as nitrates from the soil; and the results show, that the greater the amount of nitrogen assimilated the more is the ash characterised by containing fixed base in combination with carbonic acid; presumably representing organic acid in the vegetable substance before incineration. The conclusion was that, independently of any specially physiological function of the bases, such as that of potash in connection with the formation of carbohydrates, for example, their office was prominently also that of *carriers of nitric acid*, and that when the nitrogen had been assimilated, the base was left as a residue in combination with organic acid—which was represented by carbonic acid in the ash. Further existing knowledge—as to the condition in which combined nitrogen is found in soil waters, as to the action of nitrates used as manures, as to the presence of nitrates in still-growing plants, and as to the connection between the nitrogen assimilated and the composition of the ash as had been illustrated—pointed to the conclusion that, at any rate a large amount of the nitrogen of the chlorophyllous vegetation on the earth's surface was derived from nitrates; whilst, so far as this was the case, the *raison d'être* of much of the fixed base found in the ashes of plants would seem to be clearly indicated.

The various results and conclusions above referred to were found to afford material aid in the interpretation of the differences in the chemical composition of the mixed herbage of the different plots which was next considered, so far as the first crops over the first twenty years were concerned.

For the purposes of the illustrations the differently manured plots were arranged in four groups as follows —1 Plots without manure or with farmyard manure 2. Plots with nitrogenous manures alone. 3 Plots with mineral manures alone 4 Plots with nitrogenous and mineral manures together. Average results for each plot, generally for a period of eighteen years, 1856—1873, and including the percentages of nitrogen, crude ash, and pure ash, in the dry substance of the produce, also the percentage composition of the pure ash were brought together in a table, and are discussed in detail The close dependence of the chemical composition of the mixed herbage on its botanical composition, and on the character of development of the plants, was throughout illustrated. It was further shown, that the mineral composition of the mixed herbage was very directly dependent on the supplies available to the plant within the soil Indeed, when it was considered that the mixed herbage of permanent grass land includes plants of very various root-range and root-habit, and that some of them vegetate more or less almost the year round, it was not surprising to find that the composition of the produce was, upon the whole, a somewhat close reflection of the available supplies within the range of the roots It was, in fact, much more so than in the case of individual crops grown separately. Within certain limits, this was the case even with the constituents of, so to speak, less functional importance than those which more obviously determined the description of plants encouraged and the character of their development. It was at the same time obvious, that when the more functionally important constituents are available in relative abundance, those which are of less importance in this respect were taken up and retained in less amount than they otherwise would be, the result being determined in great measure by the character of growth induced.

For example, if potash be liberally available the produce is much more stemmy, and the amount of soda, of lime, and to some extent of magnesia also, will be less relatively to the potash. In defect of sufficient potash, on the other hand, more of soda, or of lime, or of both, will be taken up and retained; but the herbage will at the same time be more leafy and immature. That is to say, the constituents are not mutually replaceable in the processes of growth, but accordingly as the one or the other predominates, so will the product of growth be different

There can be no doubt, that luxuriance or vegetative activity is intimately associated with the amount of nitrogen available and taken up Further, it may be stated that chlorophyll formation to a great extent follows nitrogen assimilation. But the results relating to the increased amount of non nitrogenous substance yielded in the mixed herbage under the influence of the various manures clearly indicated that the nitrogen being taken up, and the chlorophyll formed, the carbon assimilation, and the carbohydrate formation, depended essentially on the amounts of potash available. It may be stated as a matter of fact that, in practical agriculture, artificial nitrogenous manures are chiefly used for crops containing a comparatively low percentage of nitrogen in their dry substance, and yielding comparatively low amounts of nitrogen per acre Indeed, they are mainly used for the increased production of the non-nitrogenous bodies—the carbohydrates—starch and cellulose in the cereals, starch in potatoes, and sugar in the sugar-cane and in root crops, for example And now, in the case of the mixed herbage of grass land, it was seen that, provided the mineral constituents, and especially potash, were abundantly available, a characteristic effect of nitrogenous manures was to increase the production of the non-nitrogenous bodies

"On the Orientation of the Pyramids and Temples in the Sûdân" By E. A. WALLIS BUDGE, M.A., Litt.D., D.Lit., F.S.A. Communicated by Professor Sir NORMAN LOCKYER, K.C.B., F.R.S. Received April 14, 1899

Received April 14, 1899

In the year 1897 I was sent on a mission to the Sûdân by the Trustees of the British Museum, and in 1898 I was again sent to that country to complete the work in the places which I could not reach the year before on account of the unsettled state of that unhappy land. By the favour of Viscount Cromer and Lord Kitchener, the Sirdar of the Egyptian army, I was enabled to visit sites which had not been visited by Europeans for a great many years, and, by the unusual facilities which these gentlemen afforded me, to make notes on matters of scientific interest which have, in recent years, been widely discussed. Besides the examination of the ruins of temples and the copying of the inscriptions which the hand of time had spared, my wish was to collect, so far as possible, accurate information concerning the orientation of the pyramids in the Sûdân, and to obtain measurements of them with special reference to the work which Professor Sir Norman Lockyer and Mr Penrose have done on the temples of Egypt and Greece respectively

It will be remembered that a few years ago Sir Norman Lockyer promulgated the theory that Egyptian temples and pyramids were oriented to certain stars, which were sacred to certain Egyptian divinities, and to the sun at certain points of his course Having worked through all the available material which had been collected by himself and others, he came to the conclusion that his theory was correct, and that with accurate data in his hands concerning a given temple or pyramid, the astronomer would be able to supply the archæologist with a tolerably correct idea of the date when the site was first covered by a religious or funeral edifice. In the 'Dawn of Astronomy' a number of test cases were discussed with results which convinced me of the truth of the theory, and Mr. Penrose, working on the same lines, applied it to the temples of Greece with such remarkable results that my conviction was strengthened. It must, however, be admitted that several difficulties still remain to be cleared away, but I think that these will disappear when the temples and pyramids of Egypt have been measured and surveyed according to modern requirements. For no one can fail to notice that the plans published, even those in the great work of Lepsius, present inaccuracies of a serious kind, especially when we consider that a variation of a few degrees will wreck the most careful calculation. The object of the present paper is to inquire if, and how far, the pyramids of the Sûdân are oriented according to any definite plan, and to put on record for the use of those interested in the subject such notes and figures as I was able to make.

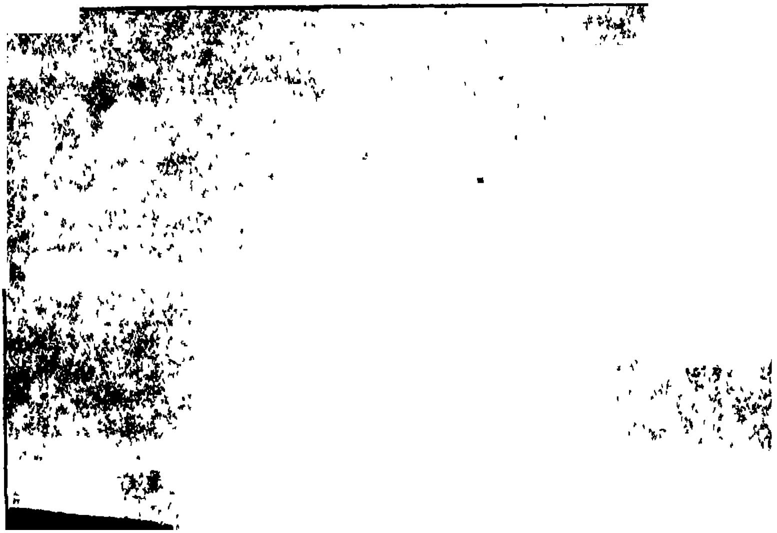
The pyramid fields of the Sûdân may be enumerated as follows:—(1) Kurru, (2) Zûmâ, (3) Tankassi, (4) Gebel Barkal, (5) Nûrî or Nawarî, (6) Merâwi, *i.e.*, the Meroé of the Greeks. The pyramids which stood upon these sites of which any remains at all exist are in number about two hundred, and it is quite certain that those which have been destroyed may be reckoned at another two hundred at least. But a pyramid field to be useful for working out the theory of orientation according to a certain plan must possess certain characteristics, such as the following:—(1) The pyramids upon it must be in a good state of preservation at their bases, and all should not, if possible, be oriented in the same direction. (2) One or more temples should be either on or near the pyramid field, so that the direction of the orientation of both kinds of buildings may be readily compared. Now, every pyramid which I have seen in the Sûdân, with the exception of those of Nûrî, consists not of a solid mass of cut stones carefully built up with a funeral chamber inside it, but of a core formed of a mixture of stones, sand, and lime which has been surrounded with a casing of stones, each measuring about 18 inches by 12 inches by 10 inches. It seems to me that the core was first built, and the casing of cut stones put round it afterwards. Curiously enough, every pyramid, with the exception of those at Nûrî, is truncated, and it is this peculiarity which has worked its ruin. For the rain has run through the flat layer of stones at the top in large quantities, and in passing between the stones at the sides, which are built without mortar, has taken with it the lime and sand from the inside; a hollow has thus been formed round the core, and the stones, aided by the furious winds which rage in the Sûdân at certain times of the year, have by their own weight fallen in upon it. Sometimes the casing has been built at too steep an angle, and the upper parts of the sides have fallen in or fallen out, as the case may be.

Yet another reason for the ruin of the Sûdân pyramids must be mentioned. The stones of which the sides are built, unlike the stones which form the pyramids of Egypt, are relatively small, and the natives have found them to be admirably adapted for certain purposes. As a result they have been filched from their places, and used to make the foundations of water-wheel supports and of houses, and also to line the shallow trenches in which the Muhammadans have buried their dead for countless generations. Thus the pyramids have, one by one, been stripped of their stone coverings, and the wind and rain together have beaten the cores so much out of shape that it is sometimes difficult, if not impossible, to distinguish them from small natural hills. The pyramids which have been built in the mountains, or at any great distance from cultivated land, are the best preserved, and this is only what might be expected. When a native wanted stones for any purpose, he went for them to the pyramid which was nearest to him, and the result is that the pyramids which stood near the villages or cultivated land have in some districts quite disappeared. Thus at Tankassi, about seven miles from Senem abû-Dôm, where the Egyptian troops were encamped about eighteen months ago, it is most difficult to identify the cores of the pyramids which once stood there. At Gebel Barkal the pyramids which were nearest to the cultivated land have disappeared, and the same may be said of dozens of the small pyramids which stood at Nûrî. At Meroe the pyramids, which were built near the temple that stood only about a mile from the river, and were in consequence close to the main road which has been the highway to Khartûm and the south for countless generations, have also all but disappeared

In this way the six pyramid fields of the Sûdân become reduced to three, for those of Kurru, Zûmâ, and Tankassi may well be left out of consideration It is, however, tolerably clear from the general disposition of the pyramid remains at these places, that the system of orientation employed by the builders of the pyramids there resembled that found to have been in use at Gebel Barkal, Nûrî, and Meroe. With the view of showing the present condition of the pyramids of the three principal fields in the Sûdân, I took about fifty photographs, one of which is reproduced in this paper. Some such record was absolutely necessary, for if the lithographic landscape views printed by Lepsius, in his work the 'Denkmaeler,' be compared with those photographs, the serious deterioration in the condition of the remains since his time will at once be clear

In the summer of 1897 I arrived at the village of Senem abû-Dôm, which is situated on the left bank of the Nile, about sixteen hundred miles from the Mediterranean, on the opposite bank lie the villages of Shibba, Merâwi, and Barkal, and on the same side as these, viz., the east bank, a few miles to the south, rises the magnificent rock of sandstone called Gebel Barkal. Before I began serious work at Gebel Barkal, I visited the pyramids of Nûrî with a view of finding out which was the more promising site. I could not visit the pyramids of Meroe that summer, because all the country round about was in the hands of the Dervishes; I therefore had to content myself with the pyramid fields of Nûrî, Barkal, Tankassi, &c ; and with the hope that I might visit Meroe later, I decided that, for several reasons, the pyramid field of Gebel Barkal suited my purpose best, and so began work there.

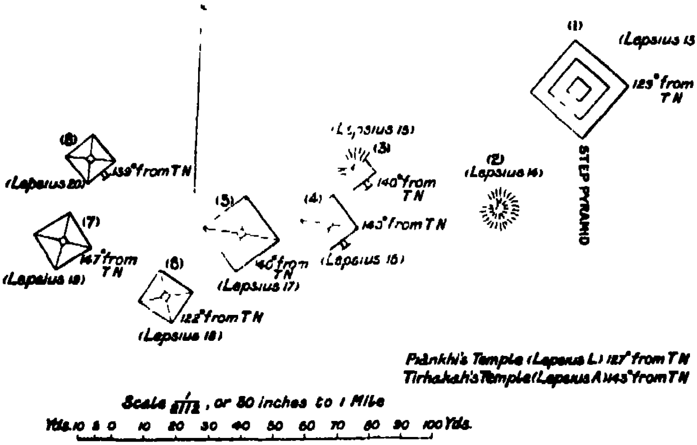
Gebel Barkal is a huge rock about three hundred feet high; it is three-quarters of a mile long, and is about half a mile wide in its widest part. The widest end has served as a quarry, and all the stones used in the casings of the pyramids for 10 miles north and south have come from it. Close under the almost perpendicular end of the mountain are the remains of a temple built by Rameses II, King of Egypt, about B.C. 1330, and those of a temple built by Piânkhi, King of Egypt and Ethiopia, about B.C. 730, and those of another built by Tirhakah, King of Ethiopia, about B.C. 680. To the south of the mountain lies the pyramid field, and the remains of the ancient city of Napata must be sought for some five or six miles further south. On the western bank of the Nile there must have stood a great city, with many temples, palaces, and other great buildings, for on several occasions when the Egyptian troops have had to build block houses and other military works, portions of large columns, pottery, &c., have been found in digging out the foundations. The site of this city is probably marked accurately by the modern village of Senem abû-Dôm, and the tombs which were made for the nobles thereof are to be found away back in the desert, at a distance of about two hours from the river, in a range of low sandstone hills.



A Pyramid at Gebel Barkal.

Of the pyramids at Gebel Barkal some are in ruins and some are tolerably complete; the former are useless for purposes of measurement, because the broken sides and the *débris* round the bases make it impossible to get accurate compass bearings. I therefore made no attempt to deal with the remains of the pyramids which are scattered about on the rocky plateau on the south side of the mountain, and I limited my inquiry to the seven which stood on the top of it. In Plan I these are set out to a scale of 30 inches to the mile, and thanks to the kindness of Colonel the Hon. M. G. Talbot, R.E., their position is very accurately indicated.* The bearings were taken with a prismatic compass, the variation of which was determined by comparison with an astronomical azimuth; but owing to the irregularities of the masonry, they cannot be relied upon to nearer than 2°. The distances were paced. The variation between the true north and the magnetic north was estimated at 5½°, and this estimate has been confirmed from

PLAN I



THE PYRAMIDS OF GEBEL BARKAL.

an examination of an Admiralty map,† which I have been so fortunate as to have had placed at my disposal. On looking at the plan, we notice that one pyramid is oriented at 129° from the true north, three at 140°, one at 122°, one at 147°, and one at 139°. Of all the temples which have been built at the foot of Gebel Barkal only two have any substantial remains, viz., those of Piânkhi and Tirhakah, the orientation of the former is 127° from the true north, and that of the latter 143°. Now it seems to me that we may fairly assume that both these kings, with their ancestors and successors, were buried near their temples, indeed from a common-sense point of view there was no place more suitable for their tombs than the neighbouring hill or mountain slope. I searched diligently, hoping that I might find some trace upon some of the blocks of stone which had formed the shrines or funeral chapels that had stood in front of their pyramids, but without success. Every pyramid at Gebel Barkal must have had such a shrine or chapel, but the size of the chapel depended upon the importance of the man whose tomb the pyramid was intended to cover Pyramids Nos 6 and 7 on the plan must, judging by the ruins, have had very large shrines enclosed by walls, and we may assume, from the absence of similar buildings in the fronts of the other pyramids, that they were royal tombs The masonry, however, and the general appearance of them somehow suggest that they were not the oldest of the group, though, arguing from archæological considerations, they should be as old as B.C 700.

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* My friend Colonel Talbot, who was employed by the Egyptian Government to make the triangulation and general survey of the Sûdân for military purposes, obtained astronomical azimuths from time to time, to determine the variation of his compass bearings, and he made use of such data in preparing the two plans which accompany this paper. He employed an azimuth compass. I did not use plumb-lines for finding the alignments, but a steel tape stretched horizontally along the general surface of the pyramid was taken as the direction of each side.

† The Admiralty map here referred to was specially prepared in the Hydrographic Department for the use of Professor Sir Norman Lockyer, and it is now in his possession. It is not a rough reconnaissance made with a prismatic compass, but contains the lines of declination as determined by compass observations made in the Mediterranean, and the Red Sea, and the adjacent waters of the Indian Ocean.

Passing to the most northerly end of the pyramid field of Gebel Barkal, we find lying there the remains of a "step" pyramid, and as it has an entirely different orientation from that of the other pyramids there, and the masonry is of a better class of work, and the whole building is on a scale *sui generis*, it is clear that it belongs to a different period. It is a striking fact that archæological considerations indicate that the pyramids which have different orientations belong to different periods, and at the end of this paper it will be seen that the results deduced from astronomical considerations point the same way.

The official instructions which I received before I went to the Sûdân in 1897 allowed me to make a trial excavation of one pyramid. I therefore selected No. 5 of my plan, as on its shrine there were sculptured scenes in which funeral offerings were being made to the royal personage who had the pyramid built, by priests, by Anubis the god of the dead, and by a number of gods whom it is not easy to identify. This royal personage was assumed to have identified himself with Osiris, and the goddesses who usually attended the god were here seen attending the king or prince. That he was royal there is no doubt, for one relief showed him in the act of grasping the hairy scalps of representatives of a number of captive or subdued nations, and brandishing a Sûdânî club over them, much in the same way as the kings of the XVIIIth and XIXth dynasties of Egypt are depicted on the walls of their tombs and palaces. To indicate the greatness and power of the king his figure had been made very large, a custom common among savage or semi-savage tribes; the figures of the vanquished were small, and were huddled together.

To make certain that the mummy chamber was not in the pyramid itself, the stones from one corner, about half way up, were removed, and a boring was made to the length of several feet, but it was soon evident that the core of the pyramid was not made of masonry, but of stone, sand, lime, &c., roughly mixed together. This being so, the hole was filled up, and the stones replaced in the casing work.

As soon as this trial work was done, by the help of the British officers, about forty men were collected, and, provided with a few tools and a good number of baskets, we began to search for the pit which led to the mummy chamber, which seemed to be below the ground. A trench was dug round all four sides, and at length a large flat slab of hard stone, 10 feet by 6 feet by 10 inches, set in lime, was found on the S.E. side of the pyramid; this was broken through, and the layers of lime and sand which came beneath showed that we had reached the mouth of the pit or shaft. We toiled through 60 feet of rough concrete in about four weeks, and at length reached a rectangular chamber about 9 feet cube, hewn, like the pit, out of the solid rock, the roof of this chamber was supported upon square pillars. A narrow passage on the S.E. side of the chamber led into another chamber which had square pillars likewise; both chambers were half filled with sand. On the sand at the foot of the shaft or pit we found some bleached bones and a broken wine jar, upon which were inscribed the words ΟΙΝΟΣ ΡΟΔΙΟΣ, *i.e.*, "Rhodian wine." The bones were like the bones of a small sheep, but they fell into dust when touched, and I could not therefore bring them home. The broken wine jar is a most interesting object, for it enables us to arrive at the date when it was put into the tomb. We know from several sources that Rhodian wine was used extensively during the latter half of the second century B.C., and the shape of the letters on the jar-neck points to that date. The jar, then, must have been taken up to Gebel Barkal from Alexandria, probably by boat, between B.C. 150 and B.C. 50, and broken in the chamber, which no doubt served as a funeral chapel, at the last feast of offerings held there. I do not think that this date is the date of the building of the pyramid, for it is a well-known fact that commemorative offerings were made in these chapels as long as funds for the purpose were forthcoming. Tombs of royal personages and of people of high rank were kept open by the priests for the express purpose of inducing relatives and friends to contribute offerings, chiefly in kind, at stated seasons of the year, and if a proof be wanted of this statement it is sufficient to refer to Diodorus Siculus,* who says in his history that he visited the halls and the chapels of the royal tombs in the Valley of the Kings at Thebes. Now we know that several of these tombs were built 1,300 years before the time of this historian, and yet they were open to the inspection of visitors, even Greeks, at that late date. What the broken wine jar shows us is that the pyramids of Gebel Barkal were not built by late native rulers who flourished under the rule of the Greeks and Romans between B.C. 300 and A.D. 350, but by native kings of the earlier dynasties who followed ancient funeral customs and rites that were known and practised as far back as the XIIth dynasty of Egypt, about B.C. 2500.

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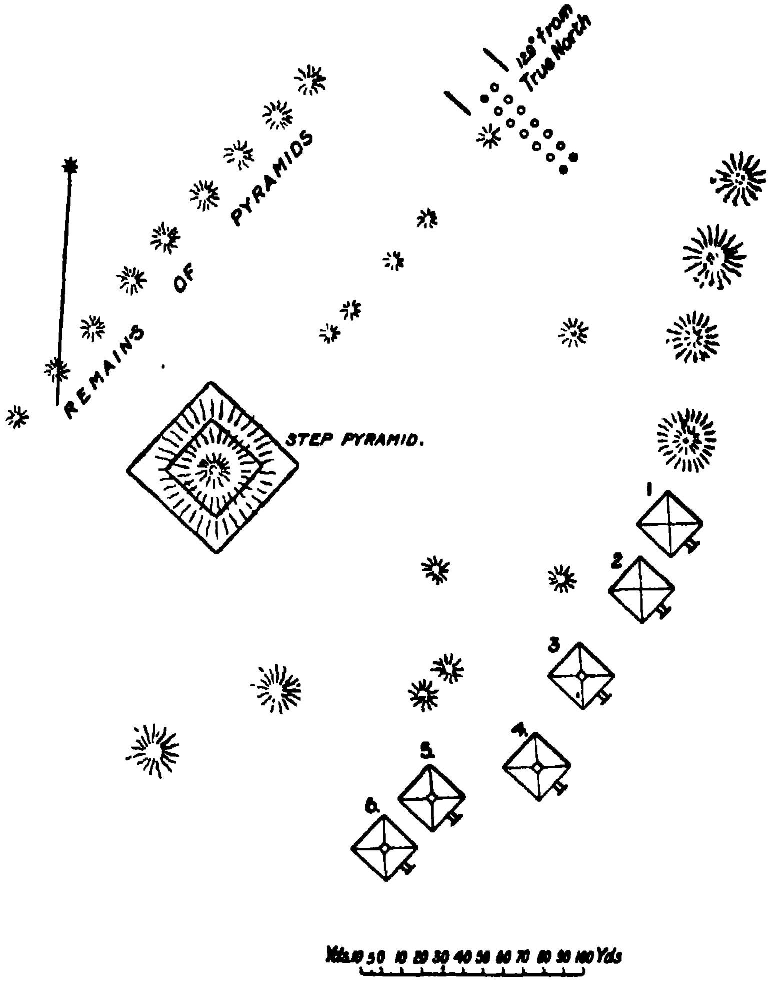
* He visited Egypt B.C. 57.

But to return to the excavation. Having removed the sand from the chambers, it became evident that neither the pillars nor the walls thereof had ever been inscribed or sculptured. A diligent search convinced me that the mummy chamber must be situated somewhere under the pyramid, and, judging from the analogy of several tombs which I excavated with General Sir Francis Grenfell, K.C.B., at Aswân in 1886–87, it ought to be exactly under its apex, if such a term may be applied to a truncated pyramid. Therefore, returning to the end of the chamber which had been hewn out of the rock immediately beneath the place where the shrine had stood, we searched for an entrance which would lead us in under the pyramid. At length we found that the wall of the chamber at this end consisted of a slab of stone about 7 feet by 6 feet by 1 foot 2 inches, embedded in lime, and when this was broken through we found a sort of vault, but there was nothing in it. The only thing that remained to do was to cut into the floor of the vault, and when we had done this we found that we had arrived at the mouth of a second pit or shaft, by which the mummy in its coffin must have been lowered into the chamber wherein it was to rest finally. We worked away at clearing out the pit by candle light under great difficulties, and when we were about 100 feet below the base of the pyramid, we began to come near to the short passage at right angles to the pit, which should have led us into the mummy chamber. At this point, however, the sides of the pit became very damp, and everything which we dug out was thoroughly wet; a foot or two lower down the men found themselves in standing water, and at length we had to stop work. We had reached a depth which had brought us down to the level of the Nile, and found that its waters had forced their way by infiltration into the shaft, and presumably into the mummy chamber also. This discovery was most disappointing, and one which I think could hardly have been foreseen. We were, therefore, obliged to be satisfied with having demonstrated the plan upon which the pyramid tomb in the Sûdân was built, and its analogy to the rock-hewn tombs of Egypt of the first twelve dynasties.

Now it is clear that, when the builders of the pyramids at Gebel Barkal selected the site for their tombs on the edge of the sandstone plateau to the south, it must have been well out of the reach of the infiltration of the Nile waters, for they knew from long experience that the presence of damp was fatal to the preservation of the mummied body. And besides, it is impossible that they should have gone on building pyramid after pyramid at various places on the plateau and on the side which slopes down to the river without finding out that the water of the Nile was making their labour vain. How then can the presence of the water be accounted for? The true answer seems to be that the bed of the Nile has gradually risen since the time when the pyramids were built, and it may be that the river has also somewhat changed its course. A small annual deposit in its bed would easily cause both the rise and the change, of course, and as a result certain sites which originally stood well above the level of the waters of the highest inundation would be flooded from time to time. The causeway leading to the river and the foremost courtyard of the temple of Piânkhi were flooded by the Nile when I was at Gebel Barkal in 1897, and that year the inundation was not by any means one of the highest. Had there been any risk of this happening in Piânkhi's time he would never have built, or rebuilt, the temple, which must have been one of the glories of the city of Napata, near the site whereof its remains now lie. Thus we see that in the Sûdân, as in Egypt, the same geological changes have been at work.

Passing now from Gebel Barkal, we have to consider the pyramids of Nûri, or Belal, which stand on the west bank of the Nile, just opposite to the now famous village of Kassingar, and at about a distance of six miles as the crow flies from Gebel Barkal. The value of the pyramid field of Nûri for a discussion on the orientation of pyramids is not so great as that of the field of Gebel Barkal, but from other points of view it is of very considerable interest. Its pyramids are not so well preserved, and there is not so much variety of orientation as is usually found in groups of pyramids elsewhere in the Sûdân. The largest of the Nûri pyramids must, with the exception of the "step" pyramids, have been the largest in the Sûdân, for when complete they cannot have been less than 100 feet high. Several of them are built of hewn stones throughout, and the excellence of the material and the handy sizes of the stones have tempted the natives to use them freely for building tombs for their sheikhs and houses and graves for themselves. Speaking roughly, the group at Nûri consists of two rows of pyramids, the older row being that in which stand the remains of a large "step" pyramid (see Plan II). The next oldest pyramids stand in a somewhat irregular row about 200 yards to the S.E., and between the two rows a number of pyramids were built at a later date, great care being taken by their builders to place them in such positions that the light from the celestial body to which they were oriented should not be obstructed by the older buildings. The first row of pyramids is in

Plan II.



The Pyramids of Nûrî.

ruins, with the exception of the "step" pyramid all are small. Of the pyramids in the second row, six are sufficiently well preserved to afford us a good idea of what they were like when complete. They must have stood upon a slightly elevated site, and they could not have been much less than 100 feet in the side; on the S.E. side each pyramid had a shrine or chapel, into the innermost part of which the light from the celestial body to which it was oriented could enter. At each end of the pyramid field was a temple, the size of which it is impossible to guess at without clearing away the tens of thousands of tons of sand with which the whole site is covered That at the N E end of the field is of considerable interest, for it shows traces of a rebuilding, during which the orientation angle was altered We have seen that the pyramid field of Gebel Barkal represented the royal necropolis of the city of Napata on the north, to what city, then, did the pyramids of Nûrî belong? In the absence of the definite knowledge which can only be obtained by excavating, we shall probably be right in regarding the pyramid field of Nûrî as the royal necropolis of the city the remains of which now lie beneath the sand at Senem-abû-Dôm. This city was certainly older than Napata, and therefore the pyramids which form the tombs of its royal rulers are older than those of Gebel Barkal or of any of the other sites which lie further to the south. A remarkable fact is that all the pyramids at Nûrî, of which tolerably accurate compass bearings can be taken, are oriented 129° from the true north, and that is the angle of orientation of the "step" pyramid at Gebel Barkal. From archæological considerations the pyramids, including those built with steps, which have this angle of orientation should be older than those which have a different angle The temples in Nubia and other countries, including Egypt, which have this angle of orientation, are as old as the period which lies between the XIIth and XVIIIth dynasties, and I therefore think that the pyramids of Nûrî, at least the oldest of them, are considerably older than all the pyramids at Gebel Barkal, with the exception of the "step" pyramid There is another point to mention. the pyramids at Nûrî are on the left bank of the Nile, like all the ancient pyramids of Egypt, and I think this is a strong proof of their great antiquity. With the making of measurements of the pyramids, and a careful examination of the ruined shrines in the hope of finding inscriptions, my work at Nûrî in 1897 came to an end.

Towards the end of last year I left England for the Sûdân, and before the end of December, thanks to the facilities afforded me by the Sirdar, I arrived at the Atbara River. In due course I was sent on to a place called Begrawîyyeh, from where I was able to examine the remains of the temples and pyramids which mark the site of the ancient city of Meroe, and of the home of the great queens who ruled there under the name of "Candace" From the spot where I landed, the ruins of the great temple of Meroe are about one and a half miles distant; by the side of them, running nearly due north and south, is the old Khartûm road, and in a line almost due east lie two groups of pyramids, at a distance of about four and a half miles from the river as the crow flies. The *shêkh* of the district, Muhammad Amîn, who had been an officer of some rank under General Gordon, gave me every assistance in his power, but the country had been so ravaged and wasted by the Dervishes led by Maḥmûd under the Khalîfa's orders, that it was with the greatest difficulty that a couple of donkeys were found for us to ride upon. The country is depopulated, and we saw hundreds of well built houses falling into ruins.

The first site visited was that of the temple of Meroe, of which portions of several pillars *in situ* still remain Before any satisfactory measurements of angles of orientation could be obtained as regards both the temple and the pyramids, I saw that a good deal of excavation and clearing away of *débris* would have to be done; but as no men could be found to do the work—there being none in the country, thanks to Dervish rule—I had to abandon that idea, and get the best results I could from the examination of the ruins only The temple appears to have been surrounded by a wall which was built at some considerable distance from it, and a very large number of people could have assembled in the space between the wall and the temple. The temple was, like most of the Sûdân temples, dedicated to the Sun-god Amen-Rā, whose visible type upon earth was the ram, and clearly the ram peculiar to that portion of the Sûdân. Of the shrine nothing remains, but a figure of a ram in hard, bluish-grey stone lay among the ruins of the pillars The pyramids, over a hundred in number, which are situated at no great distance to the north-east of the temple, are in ruins, and the masonry which still remains is of the same class and style as that of the most recent portions of the temple At two or three places in the plain round about are remains of buildings of the Roman period, and near one of these was found a small Greek inscription, which I brought home, it is now in the British Museum.

Passing from the riverside ruins we made our way due east towards a chain ot very low hills that lay in the distance, and after an hour's ride we arrived at a crescent-shaped eminence which stood with its convex side towards us. The top of the eminence was about eighty feet above the pebbly plain When I had walked round the pyramids it was easy to see that they must be divided for purposes of examination into three groups The first group stood on the crescent-shaped eminence mentioned above, and it seems as if this site originally consisted of a series of low hills, from which the tops had been cut off and thrown into the hollows between to make a level base for the pyramids. The first group consisted originally of about twenty-five pyramids The second group stood away to the south-east, and consisted originally of about twenty-two pyramids. The third group lay close to the second, and the pyramids which belong to it are about twenty in number. Of the pyramids of the first group there are abundant remains, and it is easy to see that they reproduce all the characteristics and all the angles of orientation with which we are familiar from the pyramids of Gebel Barkal and Nûrî, together with some others. Here, as at Gebel Barkal and Nûrî, and the pyramid field to the north-east of the ruins of the temple of Meroë, we find a "step" pyramid larger, better built, and better planned than the other pyramids which stand near, and in each place this "step" pyramid has an orientation angle of 129°

In Northern Egypt, as is well known, all the pyramids are oriented east and west, and observations show that in Southern Egypt the "step" pyramids are oriented not east and west, but in another azimuth facing south-east, with such an amplitude that it could not have been a question of the sunlight entering the shrine. We are therefore driven to star worship. Now, the chief pyramids in Northern Egypt date from B.C. 3800 to B.C. 2600, we may expect therefore that, as the idea of pyramid building was introduced into the south from the north, the building of the "step" pyramids must have taken place at a very early date Looking to this amplitude it has already been shown that some of the chief temples of Southern Egypt were oriented to α Centauri Taking the "step" pyramid at Gebel Barkal with an azimuth of 129°, we have an amplitude of 39° south of east, and, not taking into account refraction and the heights of the hills on the horizon, which would tend to neutralise each other, we find the declination of a star thus observed to be 35° 58′ south, a position which the star occupied B.C. 2700. These "step" pyramids then were probably built under the influence of the kings of the XIth and XIIth dynasties, who were famous for their building operations This date will also suit admirably from both an astronomical and an archæological aspect the temple of Piânkhi, which has a nearly identical amplitude We have to assume therefore that Piânkhi, like many other kings, simply restored a XIIth dynasty temple. With regard to azimuths from 143° to 150°, I find that the same star, α Centauri, might still have been observed, but in this case the building of both temple and pyramid must have taken place about the period which lies between B.C. 1200 and B.C. 700. Here, in my opinion, the astronomical determination agrees with the archæological requirements.

At Nûrî, as at the other places mentioned above, we find numbers of pyramids with the orientation angles of 122°, 140°, and 147°; and the characteristics of the pyramids having the same angle in one place are the same as those having the same angle elsewhere. At Nûrî and at Begrawîyyeh or Meroe, a considerable number of pyramids have exactly the same angle of orientation, viz., 129°, and the number is so great that it is clear that we are not dealing with a question of accident but of design. The great Lepsius came to the conclusion that the pyramids of Nûrî looked older, and were older, than the pyramids of Gebel Barkal, and my own observations made on the spot convince me that his view was right; and he might have added also that the pyramids at Meroe having the same angle of orientation as those at Nûrî are older than those which have a different angle. It has been said above that of the shrines which originally stood before the pyramids of Gebel Barkal and Nûrî, few remains exist, but this is not the case with the pyramids of Meroe, where we have the greater portion of many of their shrines still standing *in situ*. These remains show that the shrines consisted of two, and sometimes three, chambers with narrow doorways which served, like the various sights and sections of a telescope, to direct the rays of light from the celestial body to a given spot, that spot in the case of a pyramid being the centre of the shrine where a figure of the deceased was placed. On the walls of the shrines are cut in outline figures of kings, armed with bows and arrows, and swinging clubs over the heads of a mass of people who belong to captive races, and in some few cases we have been fortunate enough to find preserved the names of the kings who built them. Now these names help us to assign a date to the pyramids on which they are found, and it is thus possible to compare the results derived from astronomical calculations based upon angles of orientation, and those which are derived from archæological experience.

For purposes of convenience the so-called kings of Ethiopia have been divided into four groups —

1. The dynasty of Piânkhi which ruled in the eighth century B.C.
2. The dynasty of Tirhakah which ruled about a century later
3. The earlier kings of Meroe who ruled from about B C. 500 to the end of the Ptolemaic period
4. The later kings of Meroe who ruled from the beginning to the middle of the Roman domination over Egypt.

Of each of these groups of kings monuments, *i e.*, temples and pyramids, have been found, and there can be no doubt whatever about this, for a number of royal names belonging to each of these four groups have been found inscribed upon them. We have already seen that some of the temples and pyramids of Gebel Barkal belong to the period which lies between B C. 800 and B C 600, and it is now clear that some of the pyramids of Meroe belong to the period which lies between B C 500 and A D 200 Here, again, the orientation theory shows that any shrine built about that time would have been directed to the important south star Fomalhaut Now we may see from Dr. O. Danckwortt's important inquiries concerning the precessional change of place of forty-six fundamental stars from B.C. 2000 to A.D. 800,* that at zero time the declination of Fomalhaut was 39° south, and that it was slowly changing. So that a difference of 1° to 40° south would give us B.C 300, and to 38° would give us A.D. 200. After what I have said as to the difficulties, almost impossibilities, of determining exact azimuths, in my mind there now remains no doubt as to the time when these shrines were built. We have now to consider the date of the pyramids of Nûrî, on which no inscriptions have been found, and the date of some of the pyramids of Meroë on which also no inscriptions have been found; but before we can arrive at any conclusion on these points, we must briefly consider the question of the ancient civilisation of the Sûdân and its origin.

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* See 'Vierteljahrsschrift der astronomischen Gesellschaft,' Leipzig, 1881, p 76

In the first place we must put aside the name Ethiopian which is so often applied to it, because there is no evidence whatever to show that it is of Ethiopian origin; the term "Ethiopian" has been loosely applied to the ancient peoples of the Eastern Sûdân and their works, just as to any object the source of which was unknown the name "Phœnician" or "Hittite" has been applied in our own day The ancient tribes who lived on the east bank of the Nile from Wâdî Halfa to Khartûm were not negroes, and they had but little in common with the tribes who lived south and west of Khartûm, indeed they were not a black race at all The colour of the men's skins was red, not black, and that of their women, who did not expose themselves to the sun's rays, was of a yellowish-red Between these peoples and the ancient Egyptians a more or less friendly intercourse existed from the earliest times; otherwise how could the Egyptian officials who were sent to the Sûdân, to the district of "big trees," to bring back huge tree trunks to cut up for coffins and sarcophagi for their royal masters, have succeeded in their enterprises? Men sent upon missions of this kind must have followed the course of the river, for the shorter desert routes were quite impossible, at any rate on the return journey for men laden with baulks of timber. Still more remarkable is the fact that a high official, called Ba-ur-Tattu, in the reign of Assa, about B.C 3300, travelled as far south as the land of the pygmies, and brought back one of these folk for the king; and eighty years later Heru-Khuf, a governor of Abu, or Elephantine, did the same thing It is difficult to imagine that Egypt could have exercised any great power over the country south of Wâdî Halfa, but that there should have been a relatively brisk intercourse between Egypt and the Sûdân for trading purposes is only natural. Again, if the Egyptians made any colonies in the south, the introduction of Egyptian civilisation and religious ideas would inevitably follow, and intermarriages between Egyptian strangers and natives would take place. Moreover, the natives who visited Egypt would bring back with them new ideas, which in course of time they finally adopted, adding such modifications as their opinions dictated. The Egyptian viceroys and Sûdân princes would naturally build temples and tombs (*i.e.*, pyramids) after the manner of those they found in Egypt, but the orientation of these would be altered in accordance with the religious views of those who built them. But suitable sites for temples and pyramids are more rare in the Sûdân than in Egypt, and priests in both countries were unwilling to abandon a spot which had become associated with sacred beliefs or religious worship. For this reason they were driven to various shifts in order to make an old temple suitable for modern requirements, and when no amount of modification would suffice they eventually pulled it down, in whole or in part, and rebuilt it on the same site as the old one.

Since constant intercourse existed between Egypt and the Sûdân in very early times, and since the people of the one country were influenced greatly by those of the other, it is clear that there is no reason why certain of the Sûdân pyramids should not be as old almost as those of Egypt. On this point both astronomy and archæology agree, and I find, on comparing the tables in the 'Dawn of Astronomy' and the deductions which the author has made from them with my own observations made from an archæological standpoint, that our conclusions are identical. I may therefore say finally that we seem to be in the presence of three different sets of structures which were built at three different times. The oldest dates from the XIIth dynasty, when α Centauri was used as a warning star; the second from B.C. 1200 to B.C. 700, the same warning star being used; and, finally, a third group much later, when the star Fomalhaut could be observed on the horizon with the identical amplitude first employed. Tables have been prepared showing the various azimuth amplitudes and declinations, with corrections for refraction and for hills on the horizon, which are estimated at 1° or 2° in height, but it is not necessary to give them in this place because of the local difficulties in determining the azimuths, to which I have already referred.

I have very great pleasure in expressing my thanks to Viscount Cromer, Lord Kitchener (Sirdar of the Egyptian Army), Colonel Sir Francis Wingate, Colonel Sir Rudolf Slatin Pasha, Major-General Sir Leslie Rundle, Colonel the Hon. M. G. Talbot, R.E., Colonel W. H. Drage, D.S.O., and to many other British officers for assistance and help in the course of my work. Notwithstanding the incessant and laborious duties which devolved upon them as officers of a frontier field force, they readily and freely found time to forward my investigations, and but for their many acts of personal kindness I should have found it impossible to have completed my mission.

The following table of amplitudes, &c., I owe to the kindness of Professor Sir Norman Lockyer, K.C.B., F.R.S.

Table of Amplitudes of Pyramids.

	Lat.	No. on Dr. Budge's plans.	Orientation from north through east	Amp	Declination (south).			
					No correction.	Correction for refraction hill, 1° high	Correction for refraction hill, 2° high.	
Pyramids at Gebel Barkal (facing S.E.)	18° 30′ N.	1	129°	39	36° 33′	36° 24′	35° 58′	Step pyramid
		2	140	50	46 36	46 18	45 47	
		3	140	50	46 36	46 18	45 47	
		4	140	50	46 36	46 18	45 47	
		5	122	32	30 10	29 56	29 32	
		6	147	57	52 42	52 22	51 46	
		7	139	49	45 42	45 21	44 56	
Tirhakah's temple ..	..	..	143	53	49 14	48 55	48 23	
Piānkhi's „ ..	..	..	127	37	34 48	24 34	34 8	
Pyramids at Nûri ..	18° 24′ N	1—6	129	39	36 39	36 26	35 59	Step pyramid
			129	39	36 39	36 26	35 59	
Small temple (facing S.E.)	..	..	129	39	36 39	36 26	35 59	
Pyramids of Begrawiyyeh (facing S.E.)	17° N	..	129	39	37 0	36 47	36 22	Step pyramid.
			144	54	50 42	50 25	49 53	
		8	129	39	37 0	36 47	36 22	
			139	49	46 12	45 57	45 28	
			140	50	47 6	46 52	46 21	
			147	57	53 20	53 2	52 28	

5¼° magnetic variation allowed for in Dr Budge's orientations Latitudes taken from map Nûri taken as 11 miles south of Gebel Barkal (18° 30′)

"The Effect of Staleness of the Sexual Cells on the Development of Echinoids."

By H. M. Vernon, M.A., M.D., Fellow of Magdalen College, Oxford.

Communicated by W. F. R. Weldon, F R S Received June 27, 1899.

The effect of varying degrees of staleness of the ova and sperm of an organism upon subsequent development appears to have been very little studied, though such a condition must obviously be a factor of frequent occurrence under natural conditions. Thus in most of the Cœlentera, Echinoderms, and in some of the worms, it would seem to be a matter of chance whether the ova and spermatozoa come into contact when freshly shed, or only many hours after extrusion. In some mammals also, especially in man, the relative degree of freshness of ovum and spermatozoon at the time of fertilisation is entirely a matter of chance.

The chief connection in which the question of staleness has been hitherto studied is that of polyspermy. Thus O. and R. Hertwig found* that on crossing certain species of Echinoderms, as the ova of *Sphærechinus granularis* with the sperm of *Strongylocentrotus lividus*, more and more of the ova were fertilised up to a certain point if they were kept for an increasing number of hours in sea water, but that after this point they began to undergo polyspermy in an increasing degree, and to develop abnormally. To what precise extent is this tendency present, however, and how is it affected by the staleness of the ova on the one hand, and of the sperm on the other? Also, do the normally developing ova of stale sexual products continue to develop equally well with those from fresh products, or not? Such are the questions it is attempted to answer in this paper

The method of experiment was very simple. The ovaries and testes of ripe specimens of the Echinoid *Strongylocentrotus lividus* were shaken in jars of water, and portions of the contents of these were mixed, either immediately, or after a given number of hours. The mixed solutions were allowed to stand for an hour, and were then poured into beakers and diluted with about ten times their volume of water. Twenty-four hours later, some of the stirred up contents were introduced into a small glass cell, and a drop of corrosive sublimate solution added to kill the blastulæ and make them sink to the bottom. The numbers of normally developing blastulæ, and of abnormally developing and unsegmented ova were then counted, 300 to 500 being usually enumerated, in order to get an accurate estimate. In all cases the mixed ova from two or more ripe specimens were used, and were fertilised by the mixed sperm of two or more specimens, in order to get as average results as possible In the experiments to be subsequently described, however, in which the stale ova and spermatozoa were mixed several different times at a few hours' interval with fresh sperm and ova, as often as not only one fresh specimen was used in each case.

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* 'Experimentelle Untersuchungen über die Bedingungen der Bastardbefruchtung.' Jena, 1885.

In the subjoined table are given the results obtained in one of the most complete experiments. In this case parallel series of determinations were made, in which the ova and sperm were kept in, and after fertilisation diluted with, respectively tank water from the Aquarium, and pure water collected several kilometres from the shore of the Bay of Naples.

Time of fertilisation	Tank water		Pure sea water	
	Per cent. blastulæ.	Per cent diminution per hour	Per cent blastulæ.	Per cent diminution per hour
Directly	98·5	..	96·9	
After 6 hours ..	95·3	0·5	95·6	0·2
" 21 " .	83·2	0·8	97·2	nil
" 24 " . ..	77·9	1·8	92·7	1·5
" 27 " ..	73·2	1·6	66·5	8·7
" 30½ " ..	55·7	5·0	0·25	18·9
" 33 " .. .	36·0	7·9	0·0	0·1
" 35½ " . ..	2·2	13·5		
" 46 " ..	0·0	0·2		

It will be seen that the ova survived better in the tank water than in the pure sea water, though in two other similar series of experiments the reverse relationship, which one would naturally expect, showed itself. Of the ova fertilised immediately after shedding, one may see that respectively 98·5 and 96·9 per cent. developed to normal blastulæ On keeping, the ova in the tank water began at first to deteriorate more rapidly than those in the pure sea water, but between the 24th and 27th hours, those in pure sea water suddenly began to fall away, and after 30½ hours, only 0·25 per cent of the ova remained to undergo normal development. The ova kept in tank water, on the other hand, postponed their rapid degeneration till the 27th to the 35½th hours, or more especially till the 33rd to the 35½th hours. In order to show more strikingly the suddenness of the onset of this abnormal development on keeping the ova, another column has been added to each half of the table, giving the percentage diminution of normally developing ova per hour. For instance, after six hours development in tank water, 3·2 per cent. less of the ova developed normally, or, on an average, 0·5 per cent. per hour for each of the first six hours. This value is put, for convenience, against the "after six hours" line in the table, though it should rightly be placed between the "directly" and "after six hours" lines. The other values are arranged in the same way. We see then, that of the ova developed in tank water, from 0·5 to 1·8 per cent. per hour underwent abnormal development up to the 27th hour, but that then the percentage rapidly increased, till from the 33rd to the 35½th hours, it reached to 13·5 per cent. In the case of the ova kept in pure sea water, the result was still more striking. Thus till the 24th hour only 1·5 per cent. or less per hour developed abnormally, but from the 27th to 30½th hour, no less than 18·9 per cent

Time of fertilisation	Per cent diminution per hour		
	Tank water (1,000,000 per litre)	Tank water (49,000 per litre).	Pure water (680,000 per litre)
Directly	(71·6)	(83·9)	(86·0)
After 9 hours	0·7	0·7	0·2
20 „	3·3	6·6	0·8
24 „	5·9	1·1	13·8
29 „	0·5	0·05	3·5
32½ „	0·2	0·0	1·5
46 „	0·06		0·2

In this next table a similar series of observations is recorded, but in addition a third series of determinations was made, in which ova and sperm were kept in about twenty times as great a volume of water as was used in the other experiments. Thus it was thought that perhaps the keeping together of very large numbers of ova and of spermatozoa in small volumes of water might tend to increase the rapidity of their deterioration. As far as this single result can show, however, just the reverse is the case. Thus when only 49,000 ova per litre were kept together, the maximum rate of deterioration was reached between the 9th and 20th hours, whilst when 1,000,000 per litre (about the usual state of dilution) were kept, the maximum rate was not reached till the 20th to 24th hours.

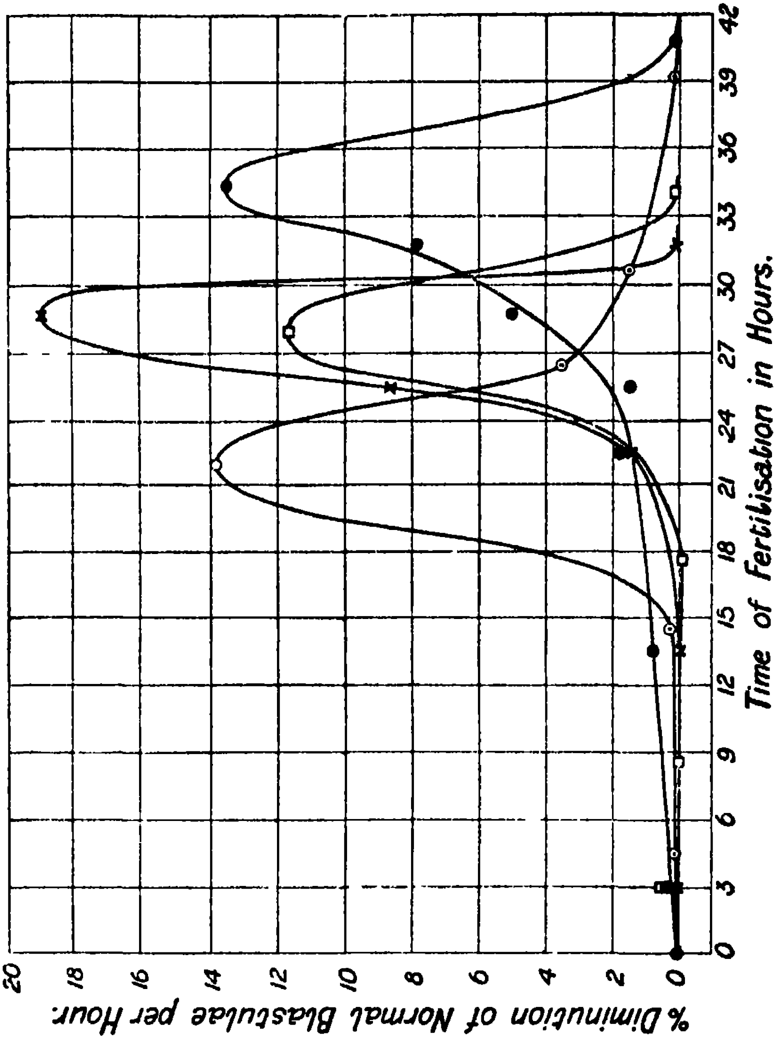
In pure sea water, with a dilution of 680,000 ova per litre, the maximum rate was also between the 20th and 24th hours, but a fair number also degenerated between the 24th and 29th hours. In this table it will be noticed that the actual percentages of blastulæ have been omitted, and only the percentage numbers of ova per hour undergoing abnormal development given. The numbers given in brackets against the "fertilised directly" line indicate the percentages of normal blastulæ produced on immediate fertilisation.

Time of fertilisation	Per cent. diminution per hour		Time of fertilisation	Per cent. diminution per hour.	Time of fertilisation.	Per cent. diminution per hour.
	Tank water	Pure water		Tank water		Tank water.
Directly....	(99·7)	(98 8)	Directly ...	(100 0)	Directly ...	(98 2)
After 6 hrs.	0·7	0·4	After 9 hrs	1 6	After 6 hrs	0·7
„ 10 „	0·6	0·4	„ 24 „	1·0	„ 11 „	nil
„ 22 „	5·8	1 3	„ 33 „	6·7	„ 24 „	nil
„ 31 „	2 2	8 6	„ 45 „	0·8	„ 32 „	11·7
„ 35 „	0·8	0 2	..	..	„ 36 „	0·1

In this next table the results of four series of observations are included. In the first two the relative effects of tank and of pure sea water, were again compared. In this case the pure water had a much better preservative effect, the ova undergoing their maximum deterioration some ten hours later than those kept in tank water. In the remaining two series of observations, the sexual products were kept in tank water, the maximum rate of deterioration being between the 24th and 33rd hours, and the 24th and 32nd hours respectively. This last experiment is in some ways the most striking one made, as 94·8 per cent. of the ova developed to blastulæ until the 24th hour, whilst by the 32nd hour only 0 8 per cent. so developed.

As a whole, therefore, these observations show a fair amount of constancy. The mean times of the period of maximum deterioration in the various series are respectively 34¼, 28¾, 22, 14½, 22, 18, 26½, 28½, and 28 hours, or, on an average, 24⅔rd hours after the shedding of the ova and sperm. The reason of this constancy may have been the similarity of the conditions of experiment. Thus all the observations were made in the latter half of March and the first half of April, and throughout the temperature of the water only varied between 13·5° and 15·3°.

The chief conclusion to be gathered from these experiments seems to me to lie in the comparative suddenness of the onset of the increased rate of deterioration of the sexual cells. Thus in all but two out of the nine experiments, the rate of increase of abnormal development remained at about 1 per cent. per hour till the 20th to the 27th hour, and then became so rapid, that within about nine hours the capacity for normal development had almost entirely disappeared. Thus rapid increase is well shown by the graphic method in the accompanying figure. Here the four most striking results obtained are reproduced, the values obtained in each of the different experiments being distinguished by different signs.



The probable reason of this increase readily suggests itself. Thus supposing that animals developing from ova which are very nearly, but not quite stale enough to avoid normal fertilisation and development, are for that reason less strong and vigorous than those arising from fresh sex cells, it follows that the period during which the sex cells remain normal ought to be as long as possible, but that, once these have begun to deteriorate, they ought to absolutely lose their functional capacity as rapidly as possible, in order that the number of enfeebled organisms which we have supposed to arise may be as small as can be.

In all the experiments thus far described, both ova and sperm were kept for similar periods before fertilisation. In order to determine whether the onset of abnormal development depended more especially on the staleness of the one element or of the other, experiments were also made in which either stale ova were fertilised with fresh sperm, or fresh ova with stale sperm. At the same time some of the stale ova were fertilised with the stale sperm, so that properly comparative results were obtained. These are collected in the subjoined table.

Time of fertilisation	Percentage of normal blastulæ		
	♀ stale ♂ stale	♀ stale. ♂ fresh.	♀ fresh. ♂ stale
After 9 hours........	88·0	81·0	97·0
„ 9 „	85·6	70·0	65·2
„ 20 „	29·0	4·7	95·5
„ 22 „	91·0	95·0	93·0
„ 24 „	45·3	87·3	96·2
„ 24 „	70·1	87·6	19·7
„ 24 „	94·4	95·0	81·1
„ 27 „	73·3	67·3	68·6
„ 29 „	2·7	94·3	5·9
„ 33 „	10·6	48·1	73·6
„ 34 „	0·0	23·8	33·0
Mean	53·6	68·6	66·5

On comparing the three columns, one can see that there is no regularity in the figures. In two cases the maximum number of normal blastulæ was obtained from stale ova and stale sperm, in four cases from stale ova and fresh sperm, and in five cases from fresh ova and stale sperm. In the last line of the table are given the mean percentages of all the observations made. From these one may perhaps conclude that whilst on an average just as many blastulæ are obtained with stale ova as with stale spermatozoa, yet that when both the sex cells are stale, the proportion is slightly less. Probably, however, one is more justified in concluding that within certain limits it is a matter of indifference whether one or other or both of the sex cells is stale. Thus if the last three observations in the table, made between the 29th and 34th hours, be omitted, the average percentages of blastulæ obtained in the remaining eight experiments are respectively 72·1, 73·5, and 77·0, *i.e.*, nearly as great with both sexual cells stale as with only one. These last three experiments would, however, seem distinctly to indicate that when the sexual cells have reached their period of rapid deterioration, it is a distinctly more favourable condition if only one and not both of the cells be stale.

The conclusion we have arrived at is not altogether an expected one, as far as one could form any expectation from indirect evidence. Thus, as before mentioned, O. and R. Hertwig found that the ova of certain Echinoids, if kept ten to twenty hours, underwent fertilisation in very much larger numbers than freshly shed ova The fresher and better the condition of the sperm, however, the better the chance of cross fertilisation. Again, in a former paper,* I showed that hybrid larvæ from the ova of *Strongylocentrotus lividus* and the sperm of *Sphærechinus granularis* could only be obtained in any number during the months of July and August, when the sexual products of *Strongylocentrotus* were found to reach their minimum maturity Those of *Sphærechinus* were, on the other hand, in a mature condition. One would therefore be inclined to conclude that in the present experiments, stale ova fertilised by fresh sperm would yield a larger proportion of blastulæ than fresh ova fertilised by stale sperm. It must be remembered, however, that the conditions are essentially different. Thus in direct fertilisation, it is the natural property of every ovum, whether fresh or stale, to undergo fertilisation by any spermatozoon which still preserves its vitality; whilst in cross fertilisation it is, as a rule, the natural property of every ovum to resist such impregnation, and this resistance is only overcome when the vitality is diminished by keeping the ovum in water, or by other means.

We have thus far examined only how far the staleness of the sexual cells affects the number of normally developing blastulæ. Does it have any more permanent effect, and do the larvæ developed from stale products differ either in form or size from those developed from fresh products? In the paper just mentioned, a few experiments upon this subject were recorded, and these showed a very distinct effect to be produced · but as they were not very numerous, no great stress was laid upon them. They have since been repeated, and sufficient confirmation of them obtained. The method of experiment was, as usual, to mix portions of the liquids containing the stale or fresh ova and sperm, and then, after an hour, pour them into jars containing about 2½ litres of sea water. These jars were kept in a tank of running sea water at a practically constant temperature for eight days, and the larvæ were then killed by the addition of corrosive sublimate, and preserved in 80 per cent. alcohol. They were then mounted in glycerin, and measured under the microscope in groups of fifty, by means of a micrometer eye-piece.† In a complete experiment, five series of measurements had to be made, viz.: (1) of the normal larvæ obtained from the fresh ova fertilised with fresh sperm, (2) those from stale ova and stale sperm, (3) from stale ova and fresh sperm obtained from another freshly opened Echinoid, (4) from fresh ova and stale sperm,

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* 'Phil. Trans.,' B, 1898, p. 465.

† For fuller details of the method, *vide* 'Phil. Trans.,' B, 1895, p. 577.

(5) and, lastly, those from the ova and sperm of the freshly opened Echinoids. It is of course impossible to get an exact basis of comparison for the larvæ obtained from one stale and one fresh sexual product. The best possible is to take a mean between the size of the original normal larvæ and that of the larvæ obtained from the fresh sexual products used for fertilising the stale products. The larvæ obtained with both sexual products stale are of course accurately comparable with the original normal larvæ.

In the accompanying table are given the mean percentage differences in the size of the larvæ obtained with fresh and stale products, from the original normal larvæ in the one case, and from the mean between the original and the fresh normal larvæ in the other two cases. The actual body length measurements of the original normal and fresh normal larvæ are also given, these being in micrometer eye-piece scale units.

Condition of sexual cells.	Fertilisation made after					
	9 hrs	24 hrs	33 hrs	45 hrs	24 hrs.	34 hrs.
Stale ♀, stale ♂ .	−0·2	+1·9	+ 1·1	− 1·9	−3·6	nil
Fresh ♀, stale ♂	+7·1	+3·7	+10·9	+ 1·5	+3·9	− 2·7
Stale ♀, fresh ♂	−2·8	−3·0	+ 2·0	−15·9	−5·2	−13·0
Body length of normal larvæ.............	29·94	..	..	..	30·72	
Body length of fresh normal larvæ.......	26·12	30·59	25·85	30·34	30·99	31·01

In the first group of observations given in the table these series of measurements were repeated after keeping the sexual products respectively 9, 24, 33, and 45 hours. When both the sexual cells were stale, it may be seen that the size of the larvæ was only slightly, if at all, affected, the average variation from the original normal larvæ being only 0·2 per cent. With fresh ova and stale sperm, on the other hand, the larvæ were considerably increased in size, even those obtained with sperm forty-five hours stale being slightly larger than the mean normal. With stale ova and fresh sperm, there was in three out of the four experiments a distinct diminution in the size of the larvæ, this amounting to no less than 15·9 per cent. in the case of the ova kept forty-five hours. This experiment thus affords a most satisfactory confirmation of the conclusion tentatively put forward in the above-mentioned paper, *i.e.*, it shows that *whilst larvæ obtained from stale ova and stale sperm are of the same size as those obtained from fresh sexual products, those from fresh ova and stale sperm are distinctly larger than the normal, and those from stale ova and fresh sperm distinctly smaller.*

cent. If, then, with a particular sea-urchin a third of the ova did not undergo fertilisation till some twenty hours stale, whilst another third underwent fertilisation by twenty hours stale spermatozoa, and only the remaining third underwent fertilisation at once by fresh spermatozoa, then the probable error of variation of all of the larvæ so arising would roughly speaking be doubled. Of course this is an extreme instance, which could never occur in the case of Echinoids, but might easily occur in the case of, for instance, man. Whether the variations so produced would be in any degree transmissible by inheritance, is quite another point, but supposing it to be only the size of the offspring which is thus influenced, it may be merely a question of varying degrees of nutrition, and so be directly transmissible. In any case, whether inheritable or not, variation of itself may be in many cases of value, as it may give a better chance to natural selection and other agencies of picking out those individuals more adapted to their environment, and rejecting those less adapted. Thus, if all the individuals are nearly alike the evolutionary process must needs be exceedingly slow

Finally, these results are of interest in that they prove the inequality of the sex cells. The diminution in the size of larvæ obtained from stale ova and fresh sperm may perhaps be looked upon as one of diminished nutrition, the result of the staleness of the yolk, but it is difficult to imagine why the staleness of the spermatozoon used to fertilise an ovum should produce a larva larger than the normal, unless one holds that the part played by the sex cells differs in some essential particular.

Summary.

The following are the chief conclusions arrived at in this paper:—

(1) If the ova and sperm of the Echinoid *Strongylocentrotus lividus* be kept for various times in sea water before fertilisation, then for about the first twenty to twenty-seven hours the number of normal blastulæ formed diminishes only about 1 per cent. per hour. After this abnormal development sets in rapidly, so that generally after a further nine hours or so, no blastulæ at all are obtained. The rate of falling off in the number of normal blastulæ may increase to as much as 18·9 per cent. per hour.

(2) If ova not more than twenty-seven hours stale be fertilised with equally stale sperm, practically as many blastulæ are obtained as when stale ova are fertilised by fresh sperm, or fresh ova by stale sperm. *After twenty-seven hours, however, the number of blastulæ obtained with both products stale falls off more rapidly.*

(3) Larvæ obtained from stale ova and stale sperm are of practically the same size as those from fresh sexual products, but those from fresh ova and stale sperm are distinctly larger than the normal, whilst those from stale ova and fresh sperm are distinctly smaller.

"On the Influence of the Temperature of Liquid Hydrogen on the Germinative Power of Seeds." By Sir WILLIAM THISELTON-DYER, K.C.M.G., C.I.E., F.R.S., Director of the Royal Botanic Gardens, Kew. Received September 28, 1899.

The 'Comptes Rendus' for August 28 (p. 434) contains a communication from Professor Dewar to M. Henri Moissan, "relative à la solidification de l'hydrogène." It concludes with the following sentence, which may be easily overlooked.—"Des graines refroidies dans de l'hydrogène liquide conservent toute la propriété de germer"

This is the first announcement of an interesting experiment in which Professor Dewar did me the honour to ask me to assist him. He has further suggested to me to put on record the facts, as far as they came under my observation, and any physiological conclusions to which they seem to point.

With this suggestion I have no alternative but to comply. Botanists will naturally expect some more detailed account than is contained in the brief announcement which I have quoted. But as my share in the research has been of the smallest, I should have much preferred that Professor Dewar should have given the result of the whole investigation himself.

When Professor Dewar first suggested the experiment to me, he pointed out that it would be a costly one, that it would only be possible to operate on very small quantities of seeds, and that the number of kinds must also be few.

The dozen seeds experimented upon by Messrs. Brown and Escombe, which were submitted to the temperature of liquid air, were apparently selected as belonging to different natural families, and also in some degree as to their composition.* My choice was much more restricted. I took two out of their list for the sake of comparison: Barley and vegetable marrow. I added wheat, which had more than once been made the subject of experiment. This gave me two farinaceous seeds and one oily one. I then took shape and bulk into account. Wheat and barley are roughly ellipsoidal and medium in size. The vegetable marrow is relatively large but flattened. I therefore added another oily seed, mustard, which is small and spherical. I followed Messrs. Browne and Escombe in taking a pea, which is also spherical in shape but nitrogenous in composition. Finally I sought a very minute seed, and pitched upon musk.

The list then ultimately stood:—

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* 'Roy. Soc. Proc.,' vol. 62, p. 161.

Brassica alba.
Pisum sativum.
Cucurbita Pepo.
Mimulus moschatus.
Triticum sativum.
Hordeum vulgare.

The next point seemed to be to eliminate the source of error which might arise from defective germinative power. I therefore communicated the list to Messrs. Sutton and Sons, of Reading, and asked their assistance With their invariable kindness in any scientific enquiry, they willingly complied, and sent the samples required with the following report :—

"We now have pleasure in sending a packet of each of the seeds you name They are all of last year's growth, and of good germination.

"For your information we append the germinations arrived at by our tests made in March last of the various parcels from which these samples are taken.

"We have no doubt that each grain of wheat is a germinating seed, as specially fine seeds have been picked out

"In the case of musk a good growth was obtained, but the germination was not counted

"Germinations —

Mustard . .	100 per cent
'Bountiful' peas	100 ,,
Vegetable marrow	96 ,,
Musk . . .	Good.
Wheat	96 ,,
Barley	100 ,, ."

I forwarded the samples (which were small) to Professor Dewar, and suggested that they should be each divided into two portions, one for a control experiment under ordinary conditions, the other to be returned to me after being subjected to cooling. Owing to some misunderstanding, this was not done; but, as will be seen in the result, the omission proved immaterial. The seeds, it should be stated, were simply air-dried: they were ordinary commercial samples, and no attempt was made to further desiccate them.

I pointed out to Professor Dewar the advisability of exposing the seeds to extreme changes of temperature as gradually as possible, a precaution which Messrs. Brown and Escombe carefully observed.* He promised "to consider what can be done to avoid any disaster from this cause."

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* *Loc. cit.*, p. 161.

On July 21 he wrote to me:—"In spite of the weather I have carried out my promise, and cooled some seeds in liquid hydrogen for half an hour. I had to seal them up in a glass tube, cool first in liquid air, and then transfer to the hydrogen. They have, therefore, been cooled to −250° C., or −252° C., while being in a vacuum (seeing the air left had no appreciable tension). The seeds, in other words, have been transferred to a condition resembling that of moving through space. Another set of the seeds have been cooled only in liquid air for comparison."

On July 22 he added, on returning the seeds:—"There can be no doubt about the seeds being cooled, as they were in the hydrogen for more than an hour. In fact I used nearly 600 c.c. of liquid hydrogen."

The seeds came to me in the small packets of tinfoil in which they had been placed in the tube. On opening these it was observed that the seeds were as fresh and bright as before being subjected to the treatment. There was not the slightest discoloration observable in the green tint of the peas. This practically disposed of the only anxiety which Professor Dewar felt as to the success of the experiment, and expressed to me on July 25:—

"My own impression is that unless the sudden vacuum caused by the liquid hydrogen cooling has produced physical rupture of the seeds, they will germinate as usual. If they survive this awful strain, then I believe no increase of the time of cooling could produce any effect other than results from one hour's exposure to such severe cold."

The seeds were sown in a cool greenhouse, without heat, on July 27. On August 1 they had all germinated. In the case of the mustard, 136 young plants were produced from 155 seeds; the remainder had, however, germinated, but the seedlings had damped off. One of the packets of wheat, for some reason, germinated slightly more slowly than the rest.

On August 5, I received a further packet of the seeds (the musk excepted) indiscriminately mixed. Professor Dewar wrote the same date:—"I have sent you seeds to-day which, if the treatment with cold can kill, ought to be dead. They have been immersed in liquid hydrogen for upwards of six hours, and no attempt was made to graduate the cooling. They were placed in the vacuum vessel into which the liquid hydrogen could drop from the apparatus, and had to take their chance. The seeds have been soaked in liquid hydrogen, and in this respect differ from the last that were cooled in a vacuum from being sealed in a glass tube."

In this instance again the seeds did not show the smallest visible trace of the ordeal to which they had been subjected. They were sorted out and immediately sown, under the same conditions as before. By August 9 the seeds had all germinated without exception. I communicated the result to Professor Dewar, and he informed me, August 15 :—"The temperature Fahrenheit to which the seeds were cooled was - 453° F. below melting ice."

These are the details of the experiment. As it is not likely to be often repeated, I have thought it worth while to place them on record as precisely as possible

The first question that suggests itself is, what evidence we have for believing that the seeds have actually been brought to the almost inconceivable temperature with which they were surrounded. That they were so brought, Professor Dewar himself has not a shadow of doubt. That substances at widely extreme temperatures can remain in juxtaposition at least for some time, and still maintain them, is illustrated by a striking experiment shown by Professor Dewar at the Royal Institution on April 1, 1898. Liquid air poured into a large silver basin heated to redness, remained apparently as quiescent at this high temperature as in cooler vessels, and maintained a spheroidal condition. This is well understood. But the fact remains that liquid air with a temperature of about - 190° C. was contained in a vessel which had a temperature of 800° C., the difference in temperature between the two being 1000° C.

If we turn for a moment to the effect of heat on living structure, we know that a temperature of 75° C. is fatal to all protoplasm, because at that temperature its proteids are coagulated. Yet there is good evidence for the fact, that seeds have been exposed for prolonged periods to a temperature above 100° C., and yet have subsequently germinated. It may be taken as absolutely certain, that in this case that temperature never reached the embryo, but must have been intercepted by the imperfect conducting power of the seed-coats. Cohn again has found that the spores of *Bacillus subtilis* survive prolonged boiling,* and a similar observation applies.

It is probable that plant structures are deficient in thermal transparency, and they are notoriously indifferent conductors. Nevertheless it is difficult to believe that in the case of such small bodies as seeds, their being brought to the temperature with which they are surrounded can be more than a question of time.

That the thermal opacity of at least the seed-coats, may be really considerable is not, however, impossible, even at low temperatures. The following remarks by Professor Dewar have an obvious bearing on this point:—

"Pictet, after an elaborate investigation, concluded that below a certain temperature, all substances had practically the same thermal transparency, and that a non-conducting body became ineffective at low temperatures in shielding a vessel from the influx of heat. Experiments, however, prove that such is not the case, the transference of heat observed by Pictet appearing to be due, not so much to the materials themselves, as to the air contained in their interstices. Good exhaustion in the ordinary vacuum vessels used in low temperature work, reduces the influx of heat to one-fifth of what is conveyed when the annular space of such double-walled vessels is filled with air."*

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* See Vine's 'Physiology of Plants,' p. 283.

It is to be noticed that in Professor Dewar's first experiment, the seeds were practically in a vacuum. It is obvious from what has been quoted above, that this would help them to retain their heat. Any hesitation in accepting the results of the experiment on this ground is however swept away by the second experiment in which the seeds with absolutely no protection at all, were actually soaked in liquid hydrogen for six hours. The extremity of caution can hardly resist the conclusion that they must have been brought to the same temperature.

Professor Dewar finds "that silica, charcoal, lampblack, and oxide of bismuth, all increase the insulation to four, five and six times that of the empty vacuum space." It might possibly be worth while to try how far a packing of small air-dry seeds would compare, say with charcoal. And this would in some degree be a measure of the thermal transparency of seed structures.

Professor Dewar suggested to me that I should supplement this statement by some remarks on the physiological bearing of the experiment. This has already been discussed by Messrs Brown and Escombe, and there is perhaps little of moment to add to their conclusions.

The real interest of the whole investigation obviously lies in the question how far it modifies our conceptions of the nature and properties of living matter. Protoplasm, whatever its source, has physical properties and an ultimate chemical composition which are practically uniform. This uniformity, however, overlies a potential diversity which is not to be measured Such diversity cannot be accounted for by any purely physical conceptions, as physical conceptions are understood.

We not merely know the ultimate constitution of protoplasm, but we also know a good deal about its proximate constitution. Yet the properties of living protoplasm are very far removed from the mere sum of those of its constituents, and no light can be derived with respect to them in this direction. And what we know about the constituent bodies themselves is at present not a little obscure. They belong, as it were, almost to the fringe of possible chemistry and almost elude the methods of chemical research. But they, complex as they are, are not themselves protoplasm. Their cumbrous molecules are built up and broken down by ordinary chemical processes. They are not in themselves, in any intelligible sense, living though essential to the exhibition of vital phenomena.

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* "On Liquid Air as an Analytic Agent," 'Roy. Inst,' Apr. 1. 1898, pp. 7 and 8.

There our analysis of living matter by physical methods for the present stops. But we are justified in pushing, at any rate, semi-physical conceptions as far as we dare. We conceive, therefore, the physical constituent molecules of protoplasm as aggregated into larger molecules which, as they are unlike anything we know as purely physical, we call physiological.* Of the properties of such molecules we have some faint conceptions. The first is their instability. They are *kinetic*, "living substance is continually breaking down into simpler bodies, with a setting free of energy; on the other hand living substance is continually building itself up, embodying energy into itself, and so replenishing its store of energy." † This kinetic condition is essentially life; when it ceases, we have hitherto believed that the constituents of protoplasm come under the sway of purely inorganic conditions.

If we pause for a moment to attempt a quasi-mechanical explanation of the more developed phenomena of living organisms, such for example as are included under heredity, we are led to suppose that the physiological molecules may themselves be grouped into larger aggregates. And each stage of aggregation introduces us into a new order of phenomena All that we can say is, that beyond the first stage the properties which are characteristic of higher molecular aggregrates, are ultra-physical, taking physical in its ordinary signification. That does not imply, however, that physical conditions are ever in abeyance. Each stage of aggregation is conditioned by every one that precedes it In this sense life rests *au fond* on a physical basis.

A continuous kinetic condition appears to be one distinctive property of physiological molecules. This not merely manifests itself in continuous chemical activity, but under appropriate conditions in actual visible motion. And it is to be remarked of the former, that though chemical in kind, it is undoubtedly ultra-chemical as chemistry is understood in the laboratory. A further characteristic of the physiological molecule is that it possesses the power of breaking up chemical combinations and reuniting their constituents in a way which absolutely eludes the methods available to the chemist, and entirely outstrides the pace at which he can proceed. There is the same kind of difference between the two methods as there is between arithmetic and the calculus in the solution of a mathematical problem.

The question then is, how far the effects of Professor Dewar's experiments and of those who have preceded him in the same field, require us to modify our conception of the physiological molecule. Are we obliged to admit with Professor C. de Candolle and Messrs. Brown and Escombe that it may descend to a purely *static* condition?

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* Identical with Foster's "semacula," 'Text-book of Physiology,' Part I, 5th ed., p. 6.

† Foster, *loc. cit.*, p. 41.

This is really bound up with another question The kinetic condition depends on the constant liberation of energy by chemical change. Of this the most important is that due to oxidation. But we now know that this is not the only source of energy in living matter, or in all cases the indispensable one. The late Dr Romanes showed that neither a high vacuum nor subsequent exposure for twelve months to absolutely indifferent gases, such as hydrogen or nitrogen, or even poisonous ones such as hydrogen sulphide, had any effect on the germinative power of seeds. Professor Pfeffer has, however, informed me in conversation that an injurious effect is ultimately produced.

Vital processes have their optimum point as regards temperature Their superior limit for the reason already pointed out is tolerably sharp; but the inferior is by no means equally so According to Boussingault the decomposition of carbon dioxide by green plants may take place nearly at 0° C.* Below the optimum there is then some evidence of a "slowing down." While some processes reach their limit, can we assume that all do?

The question would be peremptorily answered for us by those who assert that all chemical action is in abeyance at such temperatures as I am discussing. Photographic action still takes place at the temperature of liquid air, though this may be due to phosphorescence. But a jet of hydrogen will burn in it.

Messrs. Brown and Escombe sum up the two methods of explaining what has been called "dormant vitality" with sufficient accuracy in their paper. According to the one view, metabolic and its resultant kinetic activity is "slowed down" indefinitely. In such a case as now described, it might be said that this takes place along an asymptotic curve, continually approaching but never becoming equal to zero.

According to the other, protoplasm passes absolutely from the kinetic to the static condition. Its locked-up energy becomes purely potential, and Professor C. de Candolle has not hesitated under these circumstances to compare it to an explosive.

It has been pointed out that such a conclusion is absolutely in conflict with Mr. Herbert Spencer's well-known definition of life. But it appears to me that that definition was only intended to apply to higher stages of the aggregation of living matter than that of the physiological molecule on which I have endeavoured to fix the discussion. The question seems to me to be simply whether it is admissible to regard *that* as capable of being brought to an absolutely static condition.

Conceive two such molecules, one known to be living, but static, and the other dead, and both to be maintained in a condition in which they are not immediately susceptible to chemical change. What is the criterion of life? There is none. It seems to me then that the question I have propounded does not admit of any positive answer in the present state of our knowledge.

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* Sachs, 'Text-book,' 2nd ed., p 729.

A problem, perhaps somewhat scholastic, which once vexed the souls of biologists was:—Whether life was the cause of organisation or organisation of life. What is to be our answer if our starting point is no more than a possible "explosive"?

"Effects of Thyroid Feeding on Monkeys" By WALTER EDMUNDS. Communicated by Professor J. ROSE BRADFORD, F.R.S.

Received September 28, 1899.

(From the Laboratory of the Brown Institution.)

The experiments were made altogether on nine monkeys; from them, however, it would be safer to exclude three. two because pathological conditions were found in their lungs after death, and one because the animal died in so short a time (seven days) after the commencement of the treatment.

There remain then six experiments for consideration.

The thyroid preparations administered were either a powder made according to the directions of Mr. Edmund White, or thyrocolloid prepared by the method of Dr. Hutchison

The doses given were large and corresponded to from one-half to three sheep's thyroids (both lobes) to a monkey daily.

Marked effects were produced by this treatment: the eyes became abnormally prominent; the monkeys lost flesh, became weak, and eventually died.

The symptoms produced may be tabulated as follows: (1) Proptosis, (2) dilatation of pupils, (3) widening of palpebral fissures, (4) erection of hairs on head, (5) hair falling out in patches from various parts of the body, (6) paralysis of one or more limbs, (7) emaciation and muscular weakness, (8) death from asthenia

With respect to these symptoms, proptosis, dilatation of pupil, widening of the palpebral fissure and erection of the hairs on the head are effects which are produced by the stimulation of the cervical sympathetic.

As the chief object of these experiments was to determine as to exophthalmos, sketches were made of the monkeys before commencing treatment and also after; eye changes occurred in all the six monkeys, and of four of them sketches exist showing these changes; in two it was not practicable to obtain the second sketches, but there was no doubt about the eye changes in these also.

Exophthalmos, after the administration of thyroid preparations, has been observed before. Béclère, in a case of myxœdema in man in which excessive doses of thyroid were given by mistake, noticed amongst other symptoms a certain degree of exophthalmos. Ballet and Enriquet, in one of six dogs which were fed on thyroids, found distinct exophthalmos. Cunningham also obtained this symptom in rabbits and monkeys.

As the eye changes can be produced by stimulation of the cervical sympathetic, it seems reasonable to infer that thyroid extract acts in the same manner, and also that the increased secretion of the enlarged and altered thyroid of Graves's disease is in part at least the cause of the exophthalmos that occurs in that affection

The falling out of the hair in patches and its loosening generally were common symptoms, and were seen in all the monkeys

As to paralysis, in two of the experiments the hind limbs became paralysed; in one of these cases the treatment was stopped to see if any improvement followed, but it did not, and the animal died in a few days. In another experiment both arms became paralysed, this passed off in two days without the treatment being stopped

These paralyses have also been noticed before Béclère, in the patient referred to above, found one day hemiplegia with aphasia and hemi-anæsthesia, all of which disappeared in a few hours In thyroidless monkeys, Horsley has twice seen a complete hemiplegia, following an attack of clonic spasms, and passing off in a day or so In one of my thyroidless monkeys a temporary paresis of one arm occurred.

The symptoms of loss of flesh and muscular weakness occurred in all the experiments, and in all too the animal died, at periods varying from forty to 116 days from the commencement of the treatment

In two cases the feeding was stopped, but only when the symptoms were well marked; there was no improvement, and the animals died.

The main conclusion arrived at is, that in monkeys thyroid feeding, if large doses are given, produces exophthalmos

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"On the Orientation of Greek Temples, being the Results of some Observations taken in Greece and Sicily in the month of May, 1898." By F. C. Penrose, M.A., F.R.S., F.R.I.B.A., &c.

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The Cabeirion near Thebes.

My observations taken in 1892 were confirmed, and consequently the doubt which a correspondent at Athens had thrown upon the orientation of that temple* may be dismissed.

Girgenti.

For the orientations of the temples at Girgenti,† I had relied on some observations taken in 1885 by means of the sun's shadow and a plumb line, checked by data, given by various authorities. This year, in the case of three of the temples, I was able to remeasure them by solar observation with a theodolite, and also to obtain correctly the altitude of the eastern horizon, as seen from each of the temples, with the following results, viz.:—

The orientation angle of the Temple of Juno should be 262° 36′ instead of 264°, and the sun's altitude when surmounting the eastern horizon should be 1° 45′ instead of 0° 35′. The elements as recomputed are as follows, viz.:—

Girgenti. Latitude 37° 18′ 36″.

Name of temple.	Orientation angle.		Stellar elements.	Solar elements.	Name of star.
Temple attributed to Juno Lucina	262° 36′	A, amplitude of star or sun	+10° 33′ E	+7° 24′ E	α Arietis rising
		B, corresponding altitude	3° 30′	1 45	
		C, declination	+10° 15′	6 49	
		D, hour angles	6^h 14^m	7^h 21^m	
		E, depression of sun when star heliacal	—	11° 30′	
		F, R.A.	23° 56′	1^h 3^m	
		G, approximate date..	490 B.C.	Ap. 6	

This amended date of the temple's foundation falls within the Hellenic occupation of the site, and very nearly at the culminating epoch of the city's prosperity. If the sun's depression had been taken at 11°, the date would have been about 440 B.C. and if at 12° about 540. The depression at 11° 30′ seems to accord best with the practice used in temples of comparatively late foundation *

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* See 'Phil. Trans.,' A, vol. 190, 1897, p. 46.

† Same vol., p. 55.

The Temple of Hercules.

I found that the orientation angle of this temple agreed very closely with that previously given, but that the eastern horizon was higher, namely, 2° instead of 0° 35′; but if the solar depression be changed from 11° to 11° 54′, the result of the calculation will be the same, and the stellar elements and the date will remain unaltered.

The Olympeium.

I found that the orientation angle of the temple of Jupiter should be increased from 257° 35′ to 258° 44′, whilst the altitude of the eastern horizon has to be increased from 0° 35′ to 1° 55′. This will neutralize the effect of the alteration of the amplitude, and the stellar elements and the date (430 B.C.) will remain unaltered.

I obtained one additional example from an ancient site in Greece, namely, the Temple of Neptune at Calauria in the Isle of Poros; the scene of the last days of Demosthenes, who had chosen it for his place of exile on account of its commanding a view of his much loved Athens from the lofty ridge on which the temple was built. The elements of the orientation are as below

Calauria Latitude 37° 31′ 30″.

Name of temple.	Orientation angle.		Stellar elements.	Solar elements	Name of star.
Temple of Neptune	247° 5′	A, amplitude of star or sun	+ 26° 10′ E	+ 24° 53′ E	α Leonis rising
		B, corresponding altitude	+ 3° 0′	0	
		C, declination ……	+ 22° 24′	+ 19° 30′	
		D, hour angles ……	6^h 57^m	8^h 1^m	
		E, depression of sun when star heliacal	—	10°	
		F, R A. ……… …	7^h 23^m	8^h 27^m	
	.	G, approximate date..	960 B.C.	July 27	

In the list of temples given in the same volume, viz.: Vol. 190, p. 65, were ten examples of comparatively late date, and of most of

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* See p. 65 of vol. cited.

Athens. Latitude 37° 58′ 20″.

Name of temple	Orientation angle.		Stellar elements	Solar elements.	Name of star
Theseum	283° 6′	A, amplitude of star or sun	−1° 7′ E.	−10° 46′E	Spica, rising
		B, corresponding altitude	5° 30′	5° 6′	
		C, declination	+2° 30′	−5° 17′	
		D, hour angles	$5^h\ 42^m$	$7^h\ 12^m$	
		E, depression of sun when star heliacal	—	17° 30′	
		F, R A	$11^h\ 19^m$	$12^h\ 49^m$	
		G, approximate date	470 B C.	Oct 6	
New Erechtheum	265° 9′	A, amplitude of star or sun	+6° 30′ E	+7° 20′ E	α Arietis, rising
		B, corresponding altitude	4° 0′	3° 25′	
		C, declination	+10° 35′	+7° 34′	
		D, hour angles	$6^h\ 13^m$	$7^h\ 26^m$	
		E, depression of sun when star heliacal	—	12°	
		F, R A	$23^h\ 58^m$	$1^h\ 11^m$	
		G, approximate date	445 B C	April 9	
New Erechtheum continued		A, amplitude of star or sun	+1° 53′ W	+7° 30′ E	α Pegasi, setting
		B, corresponding altitude	3° 0′	3° 25′	
		C, declination	+3° 20′	+7° 34′	
		D, hour angles	$5^h\ 55^m$	$7^h\ 50^m$	
		E, depression of sun when star heliacal	—	14° 6′	
		F, R A	$21^h\ 3^m$	$10^h\ 49^m$	
		G, approximate date	450 B C	Sept 2	
New temple of Bacchus adjoining the earlier temple	255° 49′	A, amplitude of star or sun	+11° 8′ E	+14° 11′ E	α Arietis, rising
		B, corresponding altitude	3° 50′	3° 10′	
		C, declination	+11° 8′	+13° 7′	
		D, hour angles......	$6^h\ 15^m$	$8^h\ 20^m$	
		E, depression of sun when star heliacal	—	17° 22′	
		F, R A..	$0^h\ 4^m$	$2^h\ 8^m$	
		G, approximate date	340 B C	April 23	
New temple of Jupiter Olympius	270°	A, amplitude of star or sun	−1° 13′ W	0° E.	
		B, corresponding altitude	3°	4° 31′	
		C, declination	+0° 52′	+2° 46′	
		D, hour angles......	$5^h\ 48^m$	$7^h\ 5^m$	
		E, depression of sun when star heliacal	—	11°	
		F, R.A............	$11^h\ 33^m$	$0^h\ 25^m$	
		G, approximate date	174 B.C.	March 27	

Ephesus Latitude 37° 56′ 30″

Name of temple.	Orientation angle.		Stellar elements	Solar elements	Name of star
Temple of Diana as rebuilt after the fire	284° 35′	A, amplitude of star or sun	−2° 32′ E.	−11° 0′ E	Spica, rising
		B, corresponding altitude	6°	4° 55′	
		C, declination ……	+1° 42′	−5° 35′	
		D, hour angles ..	5^h 35^m	7^h 1^m	
		E, depression of sun when star helical		15° 30′	
		F, R A …… ….	11^h 25^m	12^h 51^m	
		G, approximate date	355 B C	Oct 6	

In this case the orientation follows the star, but with so considerable an increase of amplitude that it would have been available for many centuries as a warning star.

which the years of their foundation are at least approximately known historically, and which were shown to be conformable to the general rule, but required a deeper depression of the sun, than was sufficient for the distinct vision of the helical star. Of five of these examples I had not given the elements, viz. The Theseum, the later Erechtheum, the later Temples of Bacchus and Jupiter at Athens, and the great temple at Ephesus, as rebuilt after the fire. As the cases of some of these are very interesting in an archæological point of view, I here supply the elements in the same form as before.

With respect to the temple of Theseus at Athens, it appears to be possible, proceeding from the approximate date given by the orientation work, to arrive at a much closer determination of the probable exact year of the foundation (or perhaps dedication) of the temple, and at the same time to confirm the traditional name of the temple, which has been much disputed What I have called the traditional view is, that the temple was built under the influence of Cimon, who during his supremacy at Athens, in the year 469 or 468 B.C., brought from the Isle of Scyros the supposed relics of Theseus. The bones of the hero were then interred at Athens with great solemnity, and a temple or heroum was built over the grave and an annual celebration was appointed, under the name Thesea, which lasted two or three days and commenced on the seventh day of the month named Pyanepsion, a month which on the whole agrees with our October, but (owing to the practice at Athens of commencing the year with the new moon which occurred on or next after the summer solstice), when we compare the two calendars, and reckon the days of the month together, we find that they only agree together at intervals of 19 years—the

Metonic cycle. It is therefore necessary to consult lunar tables to see *in what year or years 7 Pyanepsion would agree with October 6*, and the nearer to 470 B.C. this can be, the better it would also agree with the orientation date.

The first Attic month was Hecatombaion having thirty days, in general agreement with July.

Then Metageitnion having twenty-nine days, in general agreement with August.

Then Boedromion having thirty days, in general agreement with September.

After which Pyanepsion.

The first three months with seven days of Pyanepsion numbers ninety-six days. Twenty-nine days of July with August, September and six of October also give ninety-six days. It follows that when the first of Hecatombaion agrees with the third of July, the seventh of Pyanepsion will represent the sixth of October.

This would have happened in 466 B.C. and not again until 447. The year 466, about three years subsequent to the recovery of the Theseian relics, would have given time for the building of the Naos of the temple, if not for its final completion, so that it may well have been the year of its dedication. On that year the astronomical combination would have been exact (not but that on any year of the cycle the rising sun would have nearly answered the purpose of the Thesea celebration, though not quite so perfectly). Of the two years above named which would have satisfied this condition, the earlier seems preferable, firstly on architectural grounds (derived chiefly from the greater spread of the capital of the column, compared with that of the Parthenon, which was commenced about the later of the two dates), and secondly, there is no record of Pericles, who was at that time the guiding spirit in Athens, having built a great temple in the lower city. If the earlier date be correct, the conclusion appears inevitable both from its combination with the Thesian relics in 469, and from its connection with the Thesea year after year, that the temple has been rightly named by tradition.

The orientation of the new Erechtheum corresponds with two of the principal Attic festivals, and also offers a suggestion of the exact year of its foundation or dedication. This is not drawn from the vernal sunrise, of which I have given elements in combination with a star; but from the autumnal return of the sun to the same declination, heralded by α Pegasi, which took place, touching the northern edge of the eastern incolumnium on September 2, and parallel with the axis on September 7. In the year 447, when the 2nd of September would have agreed with the 2nd of Boedromion, and the 7th September with the 7th of the same month; the former date would have been that of the great feast of the Nicoteria in honour of Minerva's contest with Neptune for the protectorate of Athens, and the other, the annual celebration of the Marathon victory.

As respects the year 447, which is one year earlier than the supposed commencement of the Parthenon, it seems appropriate because at that time Pericles would have been supreme, and it is likely that a temple of such sanctity as the Erechtheum would have called for his earliest attention. It is true that the temple remained long unfinished, but of the causes of this delay we are ignorant. The connection however of the orientation with the feasts above mentioned would have been exactly the same if the date had been 428

The apparent discrepance between the orientation date (as respect the day of the month) in the case of the Temples of Jupiter Olympius, and that which is supposed by Mommsen to have been the day of the celebration of the great feast to the supreme god (namely Munychion 19, the tenth month of the year, which in a general way corresponded with April), whereas the orientation dates give for the earlier temple March 30–31, and for the later March 27, is explained by the possibilities of the Metonic cycle, for when the Attic year began as it would in its course on July 11, the 19th Munychion would agree with March 30, or if July 8 with the 27th of March.

"Collimator Magnets and the Determination of the Earth's Horizontal Magnetic Force" By C. Chree, Sc D., LL.D., F.R.S., Superintendent of the Kew Observatory. Communicated by the Kew Observatory Committee of the Royal Society. Received May 31,—Read June 15, 1899.

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§ 1. The present paper deals with magnets employed in measuring declination and horizontal force. More than 100 collimator magnets of English make have been examined at Kew Observatory, and the record of the results forms probably a unique mine of information. So far as I know, the only use hitherto made of this has been in the compilation of a statistical paper on the mean and extreme values of the temperature and induction coefficients by the late Mr. G. M. Whipple.*

The examination of a collimator magnet at Kew Observatory consists mainly in the determination of certain "constants." These are the "temperature coefficients" q and q', the "induction coefficient" μ, and the "moment of inertia" K.

The values found for these "constants" are utilised in the construction of tables, intended for reducing the observations of horizontal force. After the tables are constructed one or two observations are made. Their primary object is to ensure that the application of the tables leads to satisfactory results, and that there are no instrumental defects; but incidentally they afford the means of determining the magnetic moment, m, of the collimator magnet, and also the value of a "constant," P, appearing in the expression

$$2\,mm''r^{-3}\,(1 + \mathrm{P}r^{-2})$$

for the couple exerted by the collimator magnet on an auxiliary magnet, moment m'', at distance r. The investigations described in the present paper have been prosecuted at intervals during the last five years, as the pressure of other work allowed. Their object has been twofold, 1° to find out whether any relationships exist between the several constants, and 2° to ascertain where our present knowledge wants amplification, and where the present tests are least satisfactory.

I shall first explain the real significance of the "constants," and describe briefly the method of determining them.

§ 2. *Temperature Coefficients.*—It is assumed that the magnetic moment, m', at a temperature of t° C., the magnet being free from external force, is connected with the moment m at 0° C. by the relation

$$m'/m = 1 - qt - q't^2 \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (1),$$

where q and q' are absolute constants for the magnet concerned. If (1) be taken for granted, q and q' can be determined by observing m' at any three convenient temperatures. These temperatures, as the experiment is conducted at Kew Observatory, lie usually within a degree or two of 0°, 18°, and 36° C. Magnets, however, destined for Arctic work are exposed to a temperature below 0° C, while magnets destined for tropical regions are exposed to a temperature over 40° C.

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* 'Roy. Soc. Proc.,' vol. 26, pp. 218—222, 1877.

The changes of temperature are made rapidly by introducing hotter or colder water into a wooden box containing the collimator magnet. Changes in the moment of this magnet, accompanying observed changes in the temperature of the water, are deduced from the variations in azimuth of an auxiliary magnet, freely suspended at a fixed distance from the deflecting magnet. In a single experiment, the cycle of temperature "hot," "mean," "cold" is repeated three times, and the mean of the three observations at each temperature is used in the final calculation. It is customary to have two completely independent experiments on different days, and to take the arithmetic mean of the values deduced for q and q' on the two occasions. The exact times are noted at which the several readings are taken, and suitable corrections are applied from the readings of the magnetic curves for variations in the horizontal force and declination.

§ 3. *Induction Coefficient.*—This is denoted by μ, and really means the temporary change in the magnetic moment of the collimator magnet due to unit change in the field (parallel to the magnet's length), it being assumed that the relation between temporary moment and strength of field is linear

The experiment* consists in observing the angles through which an auxiliary magnet is deflected out of the magnetic meridian by the collimator magnet, the latter being vertical, with its north pole alternately up and down. The vertical plane through the centres of the deflecting and deflected magnets is perpendicular to the latter's axis, but the centres of the magnets are *not* in the same horizontal plane.

The change in the inducing field being double the intensity of the vertical force at Kew is nearly 0·9 C.G.S. unit. As a rule, only one complete experiment is made, but this involves inverting and reinverting the magnet several times. Originally μ was measured in British units, so that conversion into C.G.S. units was necessary in many cases.

§ 4. *Moment of Inertia.*—This means the moment of inertia of the magnet and all its appendages, when at a temperature of 0° C., about the suspending fibre. A collimator magnet is a hollow steel cylinder, about 9⅛ cm. long and 1 cm. in external diameter, with screws cut on its inner surface at both ends. The appendages consist of two small cells, one holding a lens the other a glass scale, screwed into the ends of the magnet, and of a brass stirrup arrangement which carries the magnet and affords the means of supporting, parallel to it, an auxiliary solid brass cylinder. This brass cylinder is a regular geometrical object, whose moment of inertia can be calculated from its weight, length, and diameter. The actual inertia experiment consists in observing the times of vibration of the magnet, under the earth's horizontal force, when the auxiliary bar is in the stirrup, and when it is removed. To reduce the possible effects of variation in force or temperature, four complete series of vibrations are made, the first and fourth without, the second and third with the auxiliary cylinder. Allowance is made for the departure of the mean temperature from 0° C. It is customary to make two independent determinations of the moment of inertia, usually on different days, and in the event of serious discrepancy a third experiment is made. In all the older experiments conversion from British to C G S units was necessary.

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* A special apparatus is employed whose description would occupy undue space. The method is practically that described on pp. 151–3 of Lamont's 'Handbuch des Erdmagnetismus.'

§ 5. *Coefficient* P.—The meaning of this has been already explained generally. I need only add that in the deflection experiment the axes of the two magnets are perpendicular, and the centre of the deflected or, as it is called, "mirror" magnet lies on the axis produced of the collimator magnet. The general expression for the couple exerted in such a symmetrical position, the centres of the magnets being at distance r, is

$$2mm'r^{-3}(1+Pr^{-2}+Qr^{-4}+\ldots\ldots),$$

where P, Q, ...are constants, whose values depend on both the deflecting and deflected magnets. As r increases, the terms involving the higher negative powers of r tend to vanish relative to the first term; and in the present case it is assumed that the term involving P is the last that need be retained. When this is true we can determine P by comparing the couples answering to any two different values of r. The distances originally adopted at Kew Observatory when British units were employed were 1 foot and 1·3 feet; the distances now in use are 30 cm and 40 cm. For several reasons, I have recorded the value not of P but of P/r^2 at 30 cm., allowing in cases where British units had been used for the small difference between 30 cm. and 1 foot.

§ 6. The number of makers of collimator magnets is not large, and the differences between the patterns are mostly small. Still it seemed undesirable to wholly disregard the differences that unquestionably exist. Accordingly, in summarizing the results contained in the Observatory records, I have divided the magnets into six groups, distinguished by the letters A to F. Four of the groups, A, B, C, and E, contain magnets from one maker only, and are presumably fairly homogeneous. Group D is the most miscellaneous, containing magnets by three if not four makers; group F contains magnets from two makers only.

Table I gives the mean values of the various quantities specified for the several groups; while Table II gives the extreme values.

Table I.—Mean Values.

Group.	Number in group.	π^2K.	*m*.	$10^6 q$	$10^8 q'$.	μ.	$10^5 \times P/r^2$ at 30 cm. When +. Number.	When +. Mean value	When −. Number	When −. Mean value
A	7	3532	921	328	120	6·70	4	281	3	293
B	12	2767	672	257	119	6·03	0	—	9	430
C	82*	2755	873	349	144	5·64	40	843	31	354
D	12	2657	854	302	134	6·43	4	918	7	290
E	10	2520	837	349	156	7·56	10	655	0	—
F	7	1667	633	350	139	3·71	4	545	0	—
Totals and means	130	2711	840	335	140	5·85	62	762	50	361

Table II.—Extreme Values.

Group.		π^2K	*m*	$10^6 q$	$10^8 q'$.	μ	$10^5 \times P/r^2$ at 30 cm When +	When −
A	max.	3741	1142	510	185	8·27	374	447
	min.	3037	653	180	23	4·36	64	353
B	max.	3184	799	359	224	7·07	—	688
	min.	2334	481	211	14	5·31	—	121
C	max.	3072	1115	764	312	6·88	2520	1498
	min.	2183	462	167	19	4·19	30	10
D	max.	2825	1197	687	261	11·89	1775	619
	min.	2352	505	195	51	4·23	520	122
E	max.	2658	906	370	332	7·79	1188	—
	min.	2422	723	321	82	7·02	173	—
F	max.	1758	777	604	339	4·81	693	—
	min.	1600	451	273	35	2·57	383	—

Data were wanting for P in the case of eighteen out of the 130 collimator magnets, and there were no data for K in the case of three magnets, all of group C. In these twenty-one cases *m* was calculated from the recorded value of the angle through which the magnet deflected an auxiliary magnet at a distance of 1 foot, that being a quantity determined in the induction experiment. Old magnets, when remagnetized and retested, have been treated as new magnets and counted separately.

§ 7. The discussion of Tables I and II would have been much simplified if one had possessed a record of the weight and dimensions of the collimator magnets themselves. It is not, however, customary to measure these quantities at the Observatory, as they in no way enter into the tables used in reducing the observations.

The only data at my disposal, bearing directly on these points, are the results of special measurements recently made on six magnets, three belonging to group C, and three to group E

The results were as follows, the numbering being purely arbitrary; the lengths are in centimetres, the weights in grams.—

Table III.—Dimensions and Weights of specimen Magnets

Group	Magnet.	Length.	External diameter	Internal diameter at end	Weight magnet	Weight appendages.
		cm	cm	cm.	grams.	grams.
C	i	9·36	1·00	0·81	27·33	—
	ii	9·30	1·00	0·80	28·11	—
	iii	9·30	1·00	0·80	30·20	26·28
	Mean	9·32	1·00	0·80	28·55	—
E	iv	9·18	1·055	0·815	25·21	—
	v	9·12	1·055	0·815	25·64	—
	vi	9·13	1·03	0·80	26·02	32·41
	Mean	9·14	1·05	0·81	25·62	—

The specific gravities found for two of the magnets were approximately equal, the mean being 7·67

From this we conclude that in both groups of magnets the wall thickness is less at the extreme ends, where an internal screw is cut, than elsewhere; the mean internal diameter deduced from the data being 0·70 cm. for the magnets of group C, and 0·77 cm. for those of group E. The actual volumes of steel in the two cases were respectively 3·72 and 3·34 c.c.

There has been but little variability in the length or external diameter of the magnets of any one group; the volume however, if we may judge by Table III, is somewhat more variable. We should have more exact information on the point if K meant the moment of the magnet alone, but as matters stand, our conclusions are exposed to some uncertainty. The appendages, as we see from Table III, weigh about as much as the magnets themselves, their main mass lies however nearer to the axis of rotation. In fact, according to the measurements made on the special magnets the appendages contribute only from 25 to 30 per cent. of the value of K.

Again, the size of the appendages is determined mainly by that of the auxiliary brass bar, which according to the Observatory records is nearly constant for magnets of the same make. It is thus improbable that variations in the appendages exercise a large influence on the values of π^2K in the several groups

§ 8. Taking all the data into consideration, I conclude from Table I, that the magnets of group A are on the whole distinctly the largest, and those of group F very distinctly the smallest, that the other four groups are very fairly similar in this respect, though the average magnet of group E is almost certainly a trifle smaller than that of groups B and C. The variability in size was really one of my principal reasons for grouping the magnets The values of m and μ should increase, *ceteris paribus*, with the size of the magnets, and an analysis which overlooked the difference between groups A and F might well prove misleading. So far as individual groups are concerned, I do not think that the neglect of possible variations in the size of the magnets is likely to prove serious. My reasons for this opinion are partly based on phenomena discussed later in §§ 15 and 16.

§ 9 Before proceeding further, a second source of uncertainty must be noticed The moment of the residual magnetism in a magnet is not determined solely by the magnetic quality of the steel, it depends on the conditions under which magnetisation took place, on the time elapsed since that event, and on the usage to which the magnet has been exposed.

In the present case I have strong reasons to think that the very large majority at least of the magnets were magnetised under a nearly uniform set of conditions Until quite lately it was the invariable practice to magnetise all the magnets at the Observatory itself, and the same coil with similar battery power has been in use for the last forty years. The magnet is not placed inside the coil, but is stroked in a uniform way on the end of a very massive slightly projecting iron core. The capacity of this core to hold out a heavy pole piece has been regarded as a criterion of the battery being in proper order; and as apparent "saturation" is reached in an ordinary collimator magnet with the battery below standard condition, the test, though a rough one, appears fairly satisfactory.

There is a variety of indirect evidence as to the uniformity of the conditions. For instance, the dates of magnetisation of the ten magnets of largest m in group C, arranged in descending order of magnitude, were as follows:—1884, '66, '79, '73, '95, '84, '83, '94, '75, '89, '68.

As regards possible loss of magnetic moment, the magnets, with one or two probable exceptions, had all been magnetised but a short time before m and the various "constants" were determined.

§ 10. *Magnetic Moment.*—Taking into account the variability in size which we have decided to exist, it would appear from Table I that the capacity for assuming permanent magnetism has been nearly the same in the four groups A, C, D, and E; but the magnets of group B have been distinctly less receptive than the others. It is difficult to say exactly how the magnets of group F stand. They are clearly much below the average size, and their mean m is at least as large compared to the means in the other groups as one would expect from a comparison of the mean moments of inertia.

Table II shows the variability of m to have been much greater in each group than that of K.

The great variability in m is noteworthy, because when m is small, corrections for torsion, temporary induction, &c., increase in relative importance.

It has been the practice of late years to reject magnets showing exceptionally small magnetic moment, but the data from such rejected magnets are not included in Tables I and II.

§ 11 *Temperature Coefficients.*—Out of the whole 130 magnets there was not a single case in which the arithmetic mean of the values found experimentally for q' was negative. Thus within the experimental limits, say 0° to 36° C., the rate of change of magnetic moment with temperature invariably increased with the temperature

The mean value for q in groups C, E, and F, is practically identical, and is very distinctly larger than the mean values for groups B and D. Group B is specially remarkable for the smallness of the temperature coefficients; in fact, the largest value of q found in a magnet of this group is very little greater than the mean value in groups A, C, E, and F.

The mean value of q' is fairly similar for the different groups, but groups A and B have somewhat smaller values than the others.

The variability of both q and q' is, as we see from Table II, very considerable in all the groups except E, where q varies but little. The term $q't^2$ is in general small compared to the term qt, and consequently the probable error in the determination of q' is large. The variability of q' may for this reason be somewhat exaggerated in Table II.

§ 12. *Induction Coefficient.*—As explained above, μ is the product of the volume of the magnet into its permeability, as determined by reversing a field of about 0·44 C.G.S. unit, the magnet being possessed of a large permanent magnetic moment.

Table I shows that the size of the mean μ in the first five groups has little relation to the size of the mean m. This is significant, because μ and m should vary in the same way with the volume of the magnet.

At the same time, the exceptionally small volume undoubtedly possessed by the magnets of group F is almost certainly the cause of the small μ found in that group; and we shall probably be correct in concluding that the larger mean μ possessed by group A as compared to groups B, C, or D, is due to the possession of a larger volume by the first-mentioned group. The pre-eminence of the mean μ of group E is, however, explicable only by supposing that the magnets of that group possess a considerably larger permeability than the others; and this larger permeability, as we see from Table II, is a property not of one or two magnets, but of the whole group.

On the whole, as Table II shows, μ is much less variable in the several groups than q or q'.

The greatest recorded value of μ, viz., 11·89, occurs in group D. I feel, however, some doubts respecting this and a second large value, 9·72, found for a second magnet of the same group. The two magnets for which these values are recorded were tested in 1864; and one of them, when retested many years afterwards, had a μ less than 7. If we excluded these two magnets, we should find for group D a mean μ much the same as that of group C.

§ 13. *Coefficient* P.—In about half the cases P was determined from only one observation, and under such circumstances the probable error is considerable. Still I do not think that the mean values given in Table I can be much in error. It will be noticed that no magnet of group B gave a positive P, and that no magnet of groups E or F gave a negative P. In groups A, C, and D, positive and negative values occur in fairly similar proportions; but investigation showed a great predominance of negative values amongst the older magnets of group C, while in group D no positive value appeared in the six oldest magnets. As pointed out in § 5, P depends on the deflected or mirror magnet as well as on the collimator magnet, and I am inclined to ascribe the interesting difference between older and newer unifilars mainly to change in the pattern of the mirror magnets (see § 46 later).

As appears from either Table I or Table II, P when negative is usually numerically smaller than when positive. When an observer employs a unifilar strange to him the probable error in P is doubtless larger than when the instrument is one to which he is thoroughly accustomed, and it would not be unreasonable to attribute to this cause some of the large values recorded in Table II. It happens, however, that three of the largest values of P in group C were each based on three independent observations. The results of the individual experiments were as follows, the numbering being purely arbitrary:—

Unifilar.	i.	ii.	iii.
Values of $10^5 \times P/r^2$ at 30 cm.	+2415	−1439	+1031
	+2870	−1917	+ 872
	+2303	−1107	+ 904

The probable errors in the mean values found in these three cases are certainly sensible, but still they form only a comparatively small percentage of the value of P. Certain drawbacks attending large values in P are dealt with later.

§ 14. A general survey of Tables I and II shows merits and demerits in most of the groups of magnets. Thus in group B the temperature coefficients are exceptionally small, but on the other hand the magnets are somewhat weak. Again, in group E the magnets are exceptionally uniform in quality, but they possess exceptionally large permeability for temporary magnetism.

Relationships between Magnetic Constants.

§ 15. In attempting to determine the existence or non-existence of relationships between the magnitudes of the several magnetic constants, one naturally turns first to the large group C. To determine whether the size of the permanent magnetic moment influenced the other quantities, I arranged the eighty-two magnets of the group in sub-groups as below, and found the mean values of the several constants for each.

Table IV.—Analysis of Magnets of Group C according to Magnetic Moment.

Value of m	Number in sub-group	Mean m	Mean π^2K	Mean $10^4 q$	Mean $10^6 q'$.	Mean μ.	$\frac{\text{Mean } m}{\text{Mean } \mu}$	Mean $10^5 \times P/r^2$	
								When +	When −
>1000	11	1044	2812	346	140	5·96	175	1399	481
1000 to 950	10	973	2745	313	143	5·53	176	884	304
950 ,, 900	11	921	2702	331	135	5·72	161	849	188
900 ,, 875	9	888	2759	359	149	5·61	158	628	286
875 ,, 850	12	862	2809	352	144	5·65	153	859	690
850 ,, 800	10	827	2720	300	147	5·42	152	613	350
800 ,, 750	10	779	2791	365	156	5·57	140	757	257
<750	9	653	2684	438	137	5·58	117	472	298

It is only natural to expect a sensible departure between the means of any single property in a sub-group of ten magnets and in a whole group of eighty-two magnets, supposing the sub-group selected by pure chance. Bearing this in mind, we must, I think, conclude that no clear relationship exists between the value of m and that of K, q, q', or μ. The absence of apparent connection between m and π^2K seems strong evidence of the comparatively small variability in the actual size of the magnets. The fact that μ is nearly, if not quite, independent of m is important, because it is expedient *ceteris paribus* that m/μ should be as large as possible. If q, q', or μ had shown an appreciable tendency to vary with m we should have been led to suspect, as *à priori* probable, a tendency to alteration in these constants as the magnet grew weaker with age. The absence of any apparent connection of the kind is not, however, proof positive that no such tendency exists (see § 37 later).

§ 16. I next arranged the eighty-two magnets of group C in sub-groups, according to the value of μ, with the following results:—

Table V.—Analysis of Magnets of Group C according to Size of Induction Coefficient.

Value of μ	Number in sub-group	Mean μ.	Mean m	$\frac{\text{Mean } m.}{\text{Mean } \mu}$	Mean π^2k	Mean 10^6q	Mean $10^6q'$	Mean $10^6 \times P/\rho^2$. When +	When −
>6	26	6·32	883	140	2762	433	160	800	359
6 to 5·5	24	5·71	898	157	2701	330	133	904	245
5·5 ,, 5	19	5·25	854	163	2796	294	132	754	241
5 ,, 4	13	4·69	837	179	2774	296	148	787	631

There is here no trace of a connection between μ and K, which supports the conclusion drawn from the absence of apparent connection between m and K. There is probably a slight tendency when μ is very decidedly below the mean for m to be slightly low, but the tendency in m/μ to increase as μ diminishes is conspicuous

The large value for P, when negative, when μ lies between 5 and 4 has no real significance, the mean being based on only two magnets. The one clear and important relationship brought out by Table V is between μ and q Large values of these two constants unquestionably have a tendency to occur together in the magnets of group C Out of the twenty-six magnets whose μ exceeded 6, no less than eighteen had a q above the mean; while of the thirteen magnets whose μ fell short of 5, only two had a q above the mean As the result seems important, I submit the following analysis, showing the distribution of the different values of μ in group C.—

Table VI.

	10^6q = 150	200	250	300	350	400	500	600	700	800
All values....	....	4	11	17	19	13	7	8	1	2
From 7 to 6..	....	0	2	3	3	6	4	5	1	2
,, 6 ,, 5·5	....	2	3	3	7	5	2	2	0	0
,, 5·5 ,, 5..	....	2	3	6	5	2	0	1	0	0
,, 5 ,, 4..	....	0	3	5	4	0	1	0	0	0

The table is to be read thus: Of the eighty-two magnets, four had a value of 10^6q between 150 and 200, and of those, two had a μ lying between 6 and 5·5, while two had a μ lying between 5·5 and 5.

Table VI certainly supports the conclusion drawn from Table V.

As a further check on this conclusion, I arranged the eighty-two magnets in two sub-groups, according as q was above or below the mean, with the following result:—

Table VII.

q.	Number in sub-group.	Mean $10^6 q$	Mean $10^8 q'$	Mean μ	Mean m.	Mean $10^5 \times P/r^3$.	
						When +.	When −
Above mean ...	33	457	160	5·94	873	854	303
Below mean ...	49	276	133	5·43	873	830	387

Table VII, like the two previous tables, points to some connection between large values of μ and q. Out of the first twenty magnets, the arrangement being in descending order of q, only two had a value of μ below the mean for group C

The association of large values of μ and q in group C is not, however, without some conspicuous exceptions, for instance, the magnet coming eighteenth in the list just referred to had a μ of only 4·71

§ 17. There are several other interesting features in Table VII; *e.g.*, only about two-fifths of the magnets had a q above the mean. Again the fact, partly accidental of course, that the two sub-groups should have precisely the same mean m is strong evidence that in the magnets of group C the size of the principal temperature coefficient is independent of the capacity of the steel to retain a large magnetic moment.

The mean q' of the first sub-group of Table VII is distinctly larger than that of the second sub-group, but the evidence of a tendency in large values of q and q' to go together is not wholly conclusive. Thus out of the thirty-three magnets whose q exceeded the mean, fourteen had a q' below the mean; and two of these fourteen had the largest q's of the group. The force of such notable exceptions is weakened, however, by the consideration that an experiment which makes q slightly too big is more likely than not to make q' considerably too small.

§ 18 Groups B and E are the only ones beside C which are sufficiently numerous and homogeneous to merit analysis. In the two following tables I give the results obtained by subdividing each of these groups into two numerically equal sub-groups, according to the size, first of m, second of μ.

Table VIII.—Mean Values of Constants in Sub-groups.

Group.	Sub-group in order of m.	m.	$\pi^2 K$.	$10^6 q$	$10^8 q'$	μ	$\dfrac{\text{Mean } m.}{\text{Mean } \mu.}$	$10^5 \times P/r^3$.
B	First 6...	759	2850	273	135	6·10	124	−475
	Second 6.	584	2685	241	102	5·96	98	−395
E	First 5...	883	2529	336	162	7·56	117	+737
	Second 5.	791	2511	362	150	7·56	105	+572

Table IX.—Mean Values of Constants in Sub-groups.

Group.	Sub group in order of μ.	μ.	m	$\frac{\text{Mean } m.}{\text{Mean } \mu}$	π^2K.	$10^6 q$	$10^8 q'$	$10^5 \times P_1 r^2$
B	First 6...	6·48	674	104	2920	256	141	−487
	Second 6.	5·58	669	120	2606	257	96	−385
E	First 5...	7·70	860	112	2575	345	141	+909
	Second 5.	7·42	815	110	2465	353	171	+401

Values of P were wanting for three of the twelve magnets of group B.

In one respect Tables VIII and IX unquestionably agree with the results established for group C, they show a distinct tendency in m/μ to be large when m is large. There is, however, in Table IX no association of large values of μ and q. In the case of group B there is an apparent association of large values of both m and μ with large values of K, suggesting that the size of the magnets has exerted a slight but sensible influence. In the case of group E the differences between the various magnets are so extremely small that we could hardly hope to detect any relationship that was not very intimate and potent.

The previous part of the paper has been mainly historical and descriptive, it remains to consider the subject from a more critical standpoint.

Probable Errors in Determination of Horizontal Force due to Errors in Values of "Constants."

§ 19. Let us first assume that the methods of determining the "constants" and the formulæ employed in calculating the horizontal force are alike above suspicion, and investigate on this hypothesis the uncertainties introduced by the probable errors in the values found for the several "constants." To do this, we must consider the formulæ. The notation not already explained is as follows:—

X = horizontal component of magnetic force,

T_1 = semi-period of vibration, corrections having been applied, if necessary, for rate of chronometer and for finite arc of vibration;

Θ = torsion couple, during vibration experiment, when torsion angle unity;

u = deflection angle, during deflection experiment, when r distance apart of magnets' centres;

t = temperature during vibration experiment;

t' = „ „ deflection „ .

For clearness, I shall in what follows suppose r to be 30 cm., that being the smaller of the two distances now adopted generally.

X is deduced by combining the two formulæ—*

$$mX = \frac{\pi^2 K}{T_1^2}\left\{1+\frac{\Theta}{mX}-qt-q't^2+\frac{2\mu \operatorname{cosec} u}{r^3}\right\}^{-1} \quad \ldots\ldots \quad (2),$$

$$m/X = \left(1-\frac{P}{r^2}\right)\left\{1+\frac{2\mu}{r^3}+qt'+q't'^2\right\}\tfrac{1}{2}r^3 \sin u \quad \ldots\ldots\ldots \quad (3)$$

whence we have—

$$X^2 = \frac{\pi^2 K \,.\, 2r^{-3} \operatorname{cosec} u\,(1-P/r^2)^{-1} T_1^{-2}}{\left(1+\frac{\Theta}{mX}-qt-q't^2+\frac{2\mu}{r^3}\operatorname{cosec} u\right)\left(1+\frac{2\mu}{r^3}+qt'+q't'^2\right)} \quad \ldots \quad (4).$$

Suppose δK, $\delta\mu$, &c., to represent the errors in the values ascribed to K, μ, &c. Terms in Θ, μ, q, q' in the denominator of the expression for X^2 are always small compared to unity, and may for our present purpose be neglected in the coefficients of $\delta\mu$, δq, &c. In this way we easily find for the consequent error δX in X :—

$$\frac{\delta X}{X} = \tfrac{1}{2}\frac{\delta K}{K}+\tfrac{1}{2}\frac{\delta P}{r^2}-\delta\mu\frac{(1+\operatorname{cosec} u)}{r^3}+\frac{t-t'}{2}\{\delta q+(t+t')\,\delta q'\}-\tfrac{1}{2}\delta\left(\frac{\Theta}{mX}\right) \quad \ldots\ldots\ldots \quad (5).$$

From (5) we see noteworthy differences in the consequences of errors in the different "constants."

In the case of K it is not the absolute size of the error that counts, but the ratio it bears to the size of K; while in the case of P, μ, and the temperature coefficients it is the absolute size of the error that counts. In all cases δX increases with X, so that the *absolute* effect of a given error in any one of the "constants" is greater where the horizontal force is large than where it is small.

§ 20. In estimating the probable errors in the several constants, I have confined my attention to the cases in which the accepted results were based on two, and only two, experiments.

If $2\delta y$ be the difference between the values given by two experiments for a certain quantity, the probable error in the arithmetic mean, y, of the two determinations is

$$\delta y'' \equiv \delta y \times 0{\cdot}6745.$$

In many instances the value of a "constant" was based on only one experiment. In such cases we may reasonably assume that the single experiment was, on the average, neither better nor worse than the average experiment which formed one of the couple usually made. This would give for the probable error in the value of a "constant" based on one experiment only, the value

---

* *Cf.* Stewart and Gee's 'Elementary Practical Physics,' vol. 2, pp. 298, 307, &c., allowing for difference of notation.

$$\delta y' = (2)^{\frac{1}{2}}\overline{\delta y}'',$$

where $\overline{\delta y}''$ is the arithmetic mean of the probable errors $\delta y''$ found in the cases where two experiments were made.

In the few cases where constants were based on three independent experiments, one could, of course, have calculated the probable errors $\delta y'''$, and found their mean. The number of such cases appeared, however, too small to give a satisfactory mean value

§ 21. Taking first the moment of inertia, I examined seventy-one cases in which two independent determinations had been made. Employing $2\delta K$ to represent the difference between the two observed values, whose arithmetic mean is K, I found

$$\text{Mean } \delta K/K = 0{\cdot}00041.$$

The corresponding mean probable error is given by

$$\delta\bar{K}''/\bar{K} = 0\,000277,$$

and answering to this the probable error in X is

$$\delta X = 0{\cdot}000138X.$$

Ascribing to X the value 0·18 C G S. unit—which is not far from the present mean value in Great Britain—we should have

$$\delta X = 0\,000025, \text{ approx.}$$

Taking the same value 0·18 for X, I also determined the law of incidence of the probable error in the seventy-one cases examined.

The results were as follows:—

Table X.

(Prob. error) × 10^5 between	0	0·5	1	2	3	4	greater than
	0·5	1	2	3	4	5	5
Number of magnets ………	18	8	13	14	4	6	8

As measurements of horizontal force are usually taken to 1×10^{-5} C.G.S. unit, we see that error in the moment of inertia may be expected to affect the last significant figure, in these latitudes, in fifty-three cases out of seventy-one, or practically in two cases out of three. In one-ninth of the total number of cases the probable error in X shown by Table X reached or exceeded five units in the last significant figure.

Also it must be remembered that in equatorial regions X may be double the value assumed in Table X, and that δX in this case varies directly as X.

§ 22. The number of instances where two observations had been made of μ were fewer; I examined forty-one of these in all.

Representing by $\delta\bar{\mu}$ and $\delta\bar{\mu}''$ the mean of the semi-differences between the two observed values and the mean probable error respectively, I found

$$\delta\bar{\mu} = 0{\cdot}198, \qquad \delta\bar{\mu}'' = 0{\cdot}134.$$

The corresponding probable error in X, treating r as 30 cm., is, irrespective of sign,

$$\begin{aligned}\delta X &= X(1+\operatorname{cosec} u)(30)^{-3}\times 0\ 134,\\ &= 5\times 10^{-6} X(1+\operatorname{cosec} u), \text{ approx.}\end{aligned}$$

This is troublesome to deal with, because cosec u depends both on X and on the magnetic moment. As a first approximation we have in fact

$$\operatorname{cosec} u = X \times 30^3/2m.$$

To get an idea of the probable error arising from error in μ, suppose $X = 0\ 18$, as at Kew, and $m = 840$, as in the average new collimator magnet. This gives

$$\begin{aligned}\operatorname{cosec} u &= 3, \text{ approx.}\\ \delta X &= 0{\cdot}000004, \text{ approx.}\end{aligned}$$

Thus when there are two observations of μ the probable error will in the average case fail to affect the last significant figure, supposing X measured as usual to 1×10^{-5} C G.S.

Of course in a good many individual cases the probable error in μ, determined from two observations, was sufficient to affect the last significant figure at Kew. More often than not μ has been derived from a single experiment, and in the majority of such cases we should conclude that the probable error was large enough to affect the last significant figure in X, measured at Kew. Owing to the occurrence of a term

$$-r^{-3}X\operatorname{cosec} u \equiv -X^2/2m$$

in the expression for $\delta X/\delta\mu$, we see that where X is large, and m is small, error in μ may be very much more serious than in the case we have selected for numerical treatment.

§ 23. The influence of errors in q and q' on X is difficult to present clearly, as it depends both on the difference and the mean of the temperatures t and t' existing during the vibration and deflection experiments. If t and t' are equal, it does not matter—the fundamental formulæ being granted—how largely q and q' are in error: but this is an exceptional occurrence, especially in field observations. To consider all possible mean temperatures seemed unnecessary, and I thus confined my attention to the three cases

$$(t+t')/2 = 0, \qquad = 15^\circ\ \text{C.}, \qquad = 30^\circ\ \text{C.},$$

corresponding to which,

$$\delta q + (t+t')\,\delta q' = \delta q, \qquad = \delta(q+30q'), \qquad = \delta(q+60q').$$

In other words, I found the difference between the two observed values of the three quantities q, $q+30q'$ and $q+60q'$, treated independently, and the corresponding three independent probable errors.

The results, derived from seventy magnets, were as follows:—

Quantity	$10^6 q$	$10^6(q+30q')$.	$10^6(q+60q')$.
Mean semi-difference . . .	11·5	7·8	14·1
Mean probable error	7·8	5·3	9·5

Corresponding to this, we have for the mean probable errors in X

Mean temperature	0° C	15° C.	30° C.
δX.	$3{\cdot}9\,(t-t')\,10^{-6}$X	$2{\cdot}6\,(t-t')\,10^{-6}$X	$4{\cdot}7\,(t-t')\,10^{-6}$X.

The probable error is conspicuously less near the middle part of the temperature range covered by the actual experiment than near either limit of this range; and this is only what we should anticipate. When the mean temperature during a horizontal force observation, at Kew, is 15° C., it would in the average unifilar require a difference of fully 10°C between the mean temperatures during the vibration and deflection experiments to make the probable error in q and q' affect the fifth significant figure in X. So large a temperature difference as this need hardly ever be feared in a fixed observatory.

The result is so far comforting, but does not justify the conclusion that error in the temperature coefficients is a wholly improbable cause of error in X. In some individual cases the probable errors found for q and q' were five or six times larger than the mean. Again, in a considerable number of instances, q and q' have been derived from only one experiment. Finally it should be noticed that the probable error δX increases with X, and that on the whole X is largest in equatorial regions where the temperature is high, and consequently errors of given magnitude in q and q' most effective.

§ 24. The two terms $\frac{1}{2}(\delta\mathrm{P}/r^2)$ and $-\frac{1}{2}\delta(\Theta/m\mathrm{X})$ in (5) were included with the object of showing how errors in the values assigned to P or to the torsion affect X. I have, however, no satisfactory data as to the size of the probable errors in P or the torsion coefficient under normal conditions. The torsion coefficient varies from thread to thread, and also with the dampness of the air. It is in fact treated as variable, and is usually determined specially in each observation of X. The accuracy of the determination thus depends more on the observer than on the instrument.

P has never been regarded at Kew as a constant to be determined once for all, but observers are recommended to determine it for themselves, employing, when possible, a series of observations made about the same time and in one neighbourhood. In other words, P is treated as a quantity which probably alters slowly with the time, and which may vary with X In the Kew unifilar itself the mean value of P calculated from a year's observations varies slightly but somewhat irregularly from year to year

The degree of variation is best seen by consulting the following table.—

Table XI.

Year	$-10^5 P/r^2$ at 30 cm	Year.	$-10^5 \times P/r^2$ at 30 cm.	Year.	$-10^5 \times P/r^2$ at 30 cm.
1860	198	1886	153	1892	67
1875	185	1887	192	1893	
1879	112	1888	174	1894	140
1882	133	1889	211	1896	131
1883		1890	177	1897	111
1884	172	1891	121	1898	143
1885	95				

Each one of these results is based on a large number of observations, but I should hesitate to say that observational error plays no part in the apparent fluctuations. There are of course frequently minor magnetic disturbances during horizontal force observations, and two or three outstanding values of P sensibly affect a mean though derived from forty observations. Though the annual mean has invariably made P negative, positive values from individual observations are by no means uncommon.

If we consider that the data in the above table are based each on numerous experiments, taken at a fixed station under the best conditions, we must, I think allow that uncertainty in the P correction is a very probable source of trouble in survey work.

Criticism of Formulæ from Mathematical Standpoint.

§ 25. If θ be the angle made by the axis of the collimator magnet with the magnetic meridian at time τ, the equation of motion is

$$K' \frac{d^2\theta}{d\tau^2} + \Theta\theta + \{mX(1 - qt - q't^2) + \mu X . X\} \sin\theta = 0 \quad \ldots \quad (6),$$

where K′ is the moment of inertia of the magnet and appendages, and μ the induction coefficient at temperature t.

If θ be kept small enough this gives an isochronous vibration whose half period T_1 is given by

$$T_1^2 = (\pi^2K'/mX) \div \left(1 + \frac{\Theta}{mX} - qt - q't^2 + \frac{\mu X}{m}\right),$$

whence
$$mX = (\pi^2K'/T_1^2) \div \left(1 + \frac{\Theta}{mX} - qt - q't^2 + \frac{\mu X}{m}\right)$$

Unless θ be kept very small, a correction becomes necessary for "finite arc" of vibration; and we then encounter the difficulty that the torsion couple is $\Theta\theta$ and not $\Theta \sin\theta$.

This is rarely, however, of practical importance, except at places where X is specially small, supposing one avoids coarse suspension fibres. At Kew Θ/mX very seldom reaches 0·001, and θ need never exceed 2°; and under such conditions $\Theta \sin\theta$ may be freely written for $\Theta\theta$. The correction for "finite arc" then presents no peculiarity

A second criticism that may be passed is that (6) makes no allowance for air resistance; in the absence of experimental data I have nothing to say on this point.

§ 26. In the deflection experiment the deflecting and deflected magnets are at right angles, the latter making an angle u with the magnetic meridian.

Supposing t' the temperature, μ' the induction coefficient of the collimator magnet during this experiment, and X′ the horizontal force, we have—

$$X'/m = 2r^{-3} \operatorname{cosec} u \{1 - qt' - q't'^2 - (\mu'X'/m)\sin u\}(1 + Pr^{-2}) \ldots \quad (8),$$

assuming the term Qr^{-4} negligible.

Here r is the actual distance at temperature t' between the centres of the two magnets.

Unless a large magnetic storm is in progress—in which case an absolute observation of horizontal force is worthless—we may regard X and X′ as equal in the small terms of (7) and (8), and so may write these equations as

$$mX = (\pi^2K'/T_1^2) \div \{1 + (\Theta/mX) - qt - q't^2 + 2\mu r^{-3} \operatorname{cosec} u\},$$
$$X'/m = 2r^{-3} \operatorname{cosec} u\,(1 + Pr^{-2})(1 - qt' - q't'^2 - 2\mu' r^{-3}).$$

Thence, eliminating m, we have

$$XX' = \frac{2\pi^2K' \operatorname{cosec} u}{r^3T_1^2}\,\frac{(1 + Pr^{-2})(1 - qt' - q't'^2 - 2\mu' r^{-3})}{1 + (\Theta/mX) - qt - q't^2 + 2\mu r^{-3} \operatorname{cosec} u} \ldots \quad (9).$$

This differs from (4) in several respects; but some of these possess little real significance.

Thus we have XX' in (9) as against X^2 in (4); but this only means that the X appearing in (4) is in reality a mean between the values possessed by the horizontal force during the vibration and deflection experiments.

In actually comparing the result of an absolute horizontal force observation with the magnetogram one measures the curve ordinates at the mean times of the two experiments—times separated usually by thirty or forty minutes—and takes the mean of the two ordinates as corresponding to the X deduced from (4).

Again, the K appearing in (4) is really taken from a table which allows for variations of temperature, and the same is true of r.

§ 27. The first difference of real significance is that (9) contains $1 + Pr^{-2}$, while (4), on the other hand, has $(1 - Pr^{-2})^{-1}$. Supposing X measured as usual to five significant figures, this becomes objectionable when the value of X is affected by so much as 5×10^{-6} C.G.S. unit.

The limiting value of Pr^{-2} for which the substitution is justifiable is given by

$$X(1 - P^2 r^{-4})^{\frac{1}{2}} = X - 5 \times 10^{-6},$$

or, approximately, $$Pr^{-2} = 10^{-3}(10/X)^{\frac{1}{2}}.$$

This gives—

$$\text{For } X = 0{\cdot}18,\ Pr^{-2} = 0{\cdot}0074, \text{ approx.,}$$

$$X = 0{\cdot}36,\ Pr^{-2} = 0{\cdot}0053 \quad \text{,,} \quad .$$

The mean value found for Pr^{-2}, when P is positive, in Table I is just in excess of 0·0074, and values in excess of 0·0053 are very common. Thus the employment of $(1 - Pr^{-2})^{-1}$ in (4) is, to say the least of it, frequently unjustifiable.

As regards the possible size to which the error in question might attain, we should have in the case of the largest P given in Table II

$$\text{for } X = 0{\cdot}18, \text{ error } 0{\cdot}00006,$$

$$X = 0{\cdot}36, \quad \text{,,} \quad 0{\cdot}00011.$$

§ 28. The second difference of note between (4) and (9) is that $(1 + qt' + q't'^2 + 2\mu r^{-3})^{-1}$ occurs in the former as against

$$1 - qt' - q't'^2 - 2\mu' r^{-3}$$

in the latter.

Overlooking for the present a possible difference between μ and μ', we see that objection arises when

$$(qt' + q't'^2 + 2\mu r^{-3})^2$$

ceases to be negligible.

So far as the term in μ is concerned there is no cause for apprehension, as 0·0006 would be an exceptionally large value for $2\mu r^{-3}$.

The case of the temperature terms is less satisfactory. Taking, for example, the mean values of Table I, viz.,

$$q = 335 \times 10^{-6}, \qquad q' = 14 \times 10^{-7},$$

we have

$$\begin{array}{rccc} t' = & 10^\circ & 20^\circ & 30^\circ \\ qt' + q't'^2 = & 349 \times 10^{-5} & 726 \times 10^{-5} & 1131 \times 10^{-5} \end{array}$$

Following exactly the same procedure as in the case of Pr^{-2}, we thence conclude that the treatment applied to the temperature terms will on the average begin to introduce error, supposing $X = 0·18$, when t' reaches 20° C.

When a magnet with large temperature coefficients is used in hot weather at a place where X is large, the error due to the treatment of the temperature terms becomes very sensible. Suppose, for instance,

$$q = 6 \times 10^{-4}, \qquad q' = 22 \times 10^{-7},$$

values exceeded in one actual case, then we have

$$\text{for } t' = 35^\circ,$$
$$qt' + q't'^2 = 0·0237.$$

This gives an error of approximately 0·0001 C.G.S. unit when $X = 0·36$.

Criticism from Physical Standpoint.[*]

§ 29. Under this heading I shall discuss certain grounds of uncertainty which may affect the accuracy of the calculated values of the horizontal force. The considerations are mainly, but not exclusively, physical.

An objection to both the fundamental formulæ (7) and (8) is that they represent all the quantities involved as constant, whereas in general magnetic force and declination, temperature, and moment of magnet, are constantly altering.

In reality, however, the t appearing in (7) is the mean of two readings of a thermometer adjacent to the collimator magnet, taken, the one immediately before, the other immediately after, the vibration experiment; while the t' appearing in (8) is the mean of eight thermometer readings taken nearly simultaneously with the individual observations of the deflection angles. There is thus very fair provision for changes of temperature so long as the temperature changes slowly and continuously in one direction, provided the thermometers really record the temperature of the magnet. In a fixed observatory these conditions are probably as a rule fairly secured if the magnet and thermometer be freely exposed to the air for five or ten minutes before the experiment commences.

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[*] **November 22.—Compare a paper by Wild (which I had overlooked), 'Terrestrial Magnetism,' vol. 2, 1897, pp. 85—104.**

§ 30. Variations of horizontal force and declination—which likewise affects both vibration and deflection readings—are more serious. If small and gradual, they are of course likely to be largely eliminated, especially in the deflection experiment, where the order of the operations is specially devised with a view to this end. If, however, they are large and sudden, probably the only really satisfactory course is to reject the observation.

It might be supposed that at a fixed observatory corrections could always be applied by referring to the magnetograph curves. This, however, is not very feasible in practice. The time scale of the curves is so contracted that the time to which a particular ordinate refers is usually uncertain to the extent of at least half a minute. Again, the position at any instant of a magnet during rapid changes of field depends to an extent difficult to determine on the inertia of the magnet; and the magnets used in magnetographs and in absolute instruments differ greatly from one another both in inertia and in method of suspension. The vibration of a magnet under sudden irregular variations of horizontal force and declination is in all probability a very complicated problem.

§ 31. The possibility of obtaining satisfactory horizontal force measurements depends largely on avoiding times of serious magnetic disturbance

The shorter the time occupied by the observations, the better the chance of accomplishing this. Supposing a uniform procedure adopted, the time occupied by a vibration experiment is independent of the observer; it is shorter the stronger the magnet and the greater the horizontal force. It is thus of especial importance that collimator magnets intended for work in arctic regions, where the force is low and disturbances large and numerous, should be of high magnetic moment.

In the deflection experiment a great deal depends on the skill of the observer, and on the make of the unifilar; one observer may do in twenty minutes what occupies another for fifty. Extreme conscientiousness may be a positive demerit, owing to the excessive time spent in adjustments and readings.

A variety of other criticisms will be considered which apply to some one physical quantity, and are classified accordingly.

§ 32. *Moment of inertia.*—In default of any simple means of directly measuring the variation with temperature of the moment of inertia, this variation is calculated by assuming for the coefficients of linear expansion:—

18×10^{-6} in the auxiliary brass bar,

12×10^{-6} „ „ collimator magnet.

There is of course the uncertainty that the assumed mean values may not apply very exactly to the particular samples of brass and steel employed in any individual case; and there is the further criticism that the coefficient 12×10^{-6} is applied to a composite moment of inertia K, of which 25 to 30 per cent. is contributed by the magnet's appendages, which are mainly brass. The expansion coefficients are employed first in deducing the value of K at 0° C. from the value found at some higher temperature, and then in calculating from the value at 0° C., thus found, a table giving K at all temperatures likely to occur in actual use. Any error that may arise in this way will be larger the more remote the temperature from that existing during the special inertia experiments. Its probable magnitude cannot, however, be directly arrived at in the absence of measurements' of the expansion of specimen magnets, their appendages and auxiliary bars

A second source of uncertainty is the fact that one is obliged to assume uniform density in the auxiliary bar

§ 33. *Torsion of suspending thread.*—This is required (1) in reducing the declination experiment, (2) in the vibration experiment determining the horizontal force.

For a considerable time prior to a declination experiment at Kew, the suspending silk fibre is stretched by hanging from it a non-magnetic plummet similar in weight to the magnet and its appendages. As the magnetic meridian is very approximately known, it is easy to arrange that when the plummet is replaced by the declination magnet there shall be very little twisting of the silk, we may thus, with at least reasonable probability, regard the thread at the beginning of the experiment as practically free from torsion. After the experiment is concluded, the magnet is again replaced by the plummet, and after some hours its position is noted. In this way we can tell approximately the angle θ' through which the lower end of the thread has turned since the last declination reading was taken. It is assumed that θ' represents the amount of torsion in the silk when the experiment ended, and that this torsion was introduced gradually in the course of the experiment. As the magnet has to be variously manipulated, there is no antecedent improbability in the hypothesis; but it will, I think, be recognised that the taking $\theta'/2$ as the mean torsion angle during the experiment is an emendation whose success is likely to be variable.

To allow for $\theta'/2$ of torsion, one introduces a given twist into the thread and notes the consequent change of azimuth in the suspended magnet. The procedure adopted at Kew, is to turn the suspension head carrying the thread through 180° first in one direction then in another, noting the corresponding equilibrium positions of the magnet.

§ 34. The occasion for a torsional correction in the vibration experiment is the existence of the couple $\Theta\theta$ in (6), and consequently of the term Θ/mX in (7). The value of Θ/mX is deduced from the observed changes in the azimuth of the collimator magnet when the suspension head is turned through ± 180°. There are several possible criticisms. In the actual vibration experiment, the thread is twisted at most only a degree or two, and it is open to doubt whether a value found for Θ from twists through ± 180° is strictly applicable, even supposing the conditions otherwise identical.

There is usually more than one silk fibre in the suspension, and this increases the probability that the value found statically for Θ may not apply exactly to a vibration experiment.

Another criticism is that experiment really gives $\Theta/m'X'$, where m' and X' are the values of the magnetic moment and of the force during the torsion experiment, instead of Θ/mX, where m is the moment at 0° C. and X the force during the vibration experiment.

To judge of the effect of this, we require the relation between the error in Θ/mX and the consequent error in X. For this we have from (5)

$$\delta X = -X\tfrac{1}{2}\delta(\Theta/mX).$$

In order that δX should equal $\pm 5 \times 10^{-6}$ at Kew we would require to have

$$\delta(\Theta/mX) = \pm 5 \times 10^{-5}, \text{ approx.}$$

When one uses a suspension sufficient for the collimator magnet alone, without the auxiliary bar, one can with an average magnet get Θ/mX as low as 4×10^{-4} at Kew. Now an error of 10 per cent. in Θ/mX through neglecting the variation of m with temperature or the fluctuations of X at a fixed station is quite out of the question.

On the other hand, suspensions such as are frequently used to carry the auxiliary bar as well as the magnet, and are intended to stand a good deal of rough handling, *may* make Θ/mX at Kew as large as 25×10^{-4}. In such a case error may occasionally arise through not discriminating between m' and m.

In practice the most probable sources of error are inaccuracy in the torsion experiment and variation in Θ, owing to variation of moisture, between the vibration and torsion experiments.

§ 35. *Temperature Coefficients.*—In the method of determining temperature coefficients in vogue at Kew the collimator magnet is fixed inside a wooden box, rigidly attached to the pillar which carries the unifilar. The calculation assumes the mirror magnet—which is suspended as in the deflection experiment—to be exactly perpendicular to the collimator; but, owing to the position of the latter being fixed, this is in general only approximately true. Suppose the deflected magnet to make an angle u with the magnetic meridian, and an angle $\frac{1}{2}\pi \pm \psi$, instead of $\pi/2$, with the collimator magnet. The deflecting force F is given in terms of X by the equation

$$F/X = \sin u/\cos\psi.$$

If F alters owing to change of temperature in the collimator magnet, its increment δF and the increments $\delta\psi$, δu in ψ and u are connected by the relation

$$(F+\delta F)/X = \sin(u+\delta u)/\cos(\psi+\delta\psi).$$

But $\delta\psi$ and δu are equal and so, supposing $\delta F/F$ small, we have

$$\delta F/F = (\cot u + \tan\psi)\,\delta u.$$

The assumption, tacitly made in practice, that the magnetic axes are perpendicular is legitimate only when we may neglect

$$\tan\psi/\cot u.$$

The size of the error varies with the moment of the deflecting magnet, and also with the secular change of declination. A slight change is, I think, desirable either in the experimental arrangements or in the calculation.*

§ 36. A more strictly physical objection to the temperature experiment is that in it the changes of temperature are very sudden, whereas in actual use they are gradual. I am not aware of any experiments bearing directly on the question whether a change of from 15° C. to 40° C, occupying only a minute or two, has the same *temporary* effect on a magnet as an equal change occupying as many hours. Theoretically it would be very desirable to make the changes of temperature slow, but in practice this is troublesome, owing to the disturbing influence of changes in declination and horizontal force A change of 18° C. in the temperature of a collimator magnet at Kew seldom alters the azimuth of the deflected magnet by more than 10′, so that it is important to avoid the risk of sensible disturbances so far as possible, even though corrections are applied from the magnetic curves.

Another objection is that sudden changes of temperature are apt, like mechanical shocks, to permanently diminish the magnetic moment. It is clear that unless such permanent loss is small, satisfactory values of q and q' are unlikely to result from a calculation which assumes that the temperature effects are purely temporary.

In the normal temperature experiment at Kew the cycle "hot," "mean," "cold" is repeated three times, and the arithmetic mean of the three readings of the deflected magnet's azimuth, answering, say, to the three "hots" is attributed to the temperature which is the arithmetic mean of the three observed temperatures This method probably eliminates to a considerable extent the influence of permanent changes, supposing them to exist, but it also renders their detection somewhat difficult. To investigate the matter I took a number of temperature experiments, and employing the values found for q and q' calculated

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*** November 7.—Since this was written the small necessary change in the apparatus has been effected.**

the values of the magnetic moment, reduced to a common temperature, at each of the three "hot," "mean," and "cold" readings. Thus, for instance, if the three "hot" temperatures were 35°, 36°, and 37°, the observed magnetic moments were reduced to the mean temperature 36°. The departures from the mean temperatures in this case are so small that trifling errors in q or q' are immaterial Corrections were carefully applied in all cases from the magnetograms for changes in the horizontal force and declination.

In all, sixteen experiments made on ten different magnets were dealt with; the mean results are given in Table XII. By $(-\delta m/m)$ is meant the diminution found in the magnetic moment divided by the original value of the moment at atmospheric temperature. This is multiplied by 10^6 to avoid decimals.

Table XII.—Mean Values of $(-\delta m/m) \times 10^6$

Temperature	2nd reading – 1st.	3rd reading – 2nd.	3rd reading – 1st.
"Hot" .	266	95	383
"Mean".	260	75	346
"Cold"	166	56	229
Mean of 3 stages .	231	75	319

In one case only one cycle was taken.

One cannot measure m accurately to one part in 100,000, much less to one part in a million, so that a high degree of accuracy can hardly be claimed for the above figures. It seems, however, perfectly clear that normally there is a loss of magnetic moment, but that at the same time the average loss is very small

These conclusions were supported by a large number of other cases which I examined, though less carefully.

On the average, according to Table XII, a complete temperature experiment causes a loss of about 0·04 per cent in the value of m, and of this loss much the greater part occurs during the first temperature cycle.* The true reversible temperature variation of m during a cycle averages very sensibly over 1 per cent; thus the shock effect can cause but little error in the values of q and q' in the average magnet.

Cases occur, however, in which the loss of magnetic moment is very considerable, and the values of q and q' may have suffered in consequence. The property seems to depend on the tempering, not on the chemical character of the steel; for various magnets which have been rejected for defective retentiveness have behaved normally after further treatment by the maker.

In some of the experiments summarized in Table XII there was a slight apparent increase of moment at one or more of the stages. In fact, in an old magnet not recently remagnetised, and in a new magnet which had been put through a previous temperature experiment only three days before, the moment appeared greater on the third occurrence of the "hot" temperature than on the first.

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* It is intended in future to have four temperature cycles, discarding the results of the first cycle.

In investigating these permanent changes of magnetic moment I received valuable assistance from Mr R. S. Whipple, then assistant at Kew Observatory.

§ 37. There are several other sources of uncertainty affecting temperature coefficients. For instance, a small fall of temperature may not always have an effect exactly equal and opposite to that of an equal rise.

Again, it is conceivable that the values of q and q' may depend to some extent on the age or strength of the magnet, and so may alter in course of time. On this point I have some interesting data.

In 1894 three collimator magnets after long absence in India were sent to Kew Observatory to be reported on The opportunity was taken to redetermine their temperature coefficients—originally determined in 1865—before sending them for repair to the maker. After their return they were remagnetised and tested as usual.

Table XIII gives the values deduced for

$$-\frac{1}{m}\frac{dm}{dt} \equiv q + 2q't$$

at 0°, 15°, and 30° C, employing the values found for q and q'.

Table XIII

Date.	1865.			1894			1895		
Temperature	0°	15°	30°	0°	15°	30°	0°	15°	30°
Magnet i.........	240	264	288	252	285	318	347	363	380
" ii.........	219	234	250	158	200	243	—	—	—
" iii.........	290	359	429	327	366	405	363	407	451
Mean for all......	250	286	322	246	284	322	—	—	—
" " i and iii.	265	312	359	289	326	362	355	385	415

Table XIV gives particulars as to the values of the moments of these magnets at the times of the temperature experiments, and the loss of moment which occurred between 1865 and 1894.

The numbering of the magnets is arbitrary, but is the same in both tables. Magnet ii was discarded in 1894, being slightly chipped. It will be seen that the agreement between the mean temperature effects in 1865 and 1894 could hardly be better, notwithstanding the large diminution in the magnetic moments.

Table XIV.

Magnet	Moment in 1865	Moment in 1894.	Per cent. loss in 29 years.	Moment in 1895.
i	936	568	39	863
ii	776	464	40	—
iii	945	718	24	855

On the other hand, the values found for q and q' in 1895—values based in each case on two consistent experiments—are unmistakably higher than those found in 1865 and 1894. There is no apparent reason for this except the fact that the magnets had been remagnetised. The makers, in reply to inquiries, stated explicitly that "the magnets . . . were only cleaned."

We seem driven to the conclusion that whilst a gradual loss of magnetic moment *may* not appreciably influence the values of the temperature coefficients, the remagnetisation of a magnet does influence these coefficients, in at least some cases

§ 38. *Induction Coefficient.*—The method of determining μ has been already described in § 3. There are here also several uncertainties.

The assumption that the relation between the temporary moment and the field is strictly linear may not be sufficiently exact; 0·44 C.G.S. unit is a considerably stronger field than Lord Rayleigh found to limit the linear part of his induction curves, and recent German observers seem disposed to narrow his limits. The accuracy of the assumption is doubtless less in some collimators than in others, but I have no data on which to proceed.

*Again, it is open to doubt whether a magnet possessed of a large permanent moment responds equally to the action of two small fields, one tending to increase, the other tending to diminish, the total moment. This uncertainty enters into the application of μ as well as into its original determination. For in the vibration experiment the temporary and permanent magnetisms are in the same sense, whereas in the deflection experiment they are in opposite senses.

If the presence of a permanent magnetic moment influences the value of μ, then the value found at Kew, even if otherwise above criticism, will presumably become less exact as the moment falls off.

Another source of uncertainty is the probability that μ varies with the temperature.

We can best judge of the possible influence of these sources of uncertainty by reference to the formula

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* Lamont, 'Handbuch des Erdmagnetismus,' pp. 149—150, says μ is less for strengthening than for weakening fields, especially when the permanent moment is very large.

$$\delta \mathrm{X}/\mathrm{X} = -\delta\mu r^{-3}\,\mathrm{cosec}\,u - \delta\mu' r^{-3},$$

connecting the error δX in X with the departures $\delta\mu$ and $\delta\mu'$ of μ from its assumed value during the vibration and deflection experiments.

As cosec u exceeds 1, we see that if $\delta\mu$ and $\delta\mu'$ be equal, the first term on the right of the equation is the larger of the two; thus an error of given amount in μ has most effect in the vibration experiment.

We have approximately

$$r^{-3}\,\mathrm{cosec}\,u = \mathrm{X}/2m,$$

and putting $r = 30$, we should find for an average collimator magnet at Kew

$$\delta \mathrm{X} = -10^{-6}(20\delta\mu + 7\delta\mu'),\ \text{approx}$$

The average value of μ in Table I is 5·85, thus if $\delta\mu$ and $\delta\mu'$ exceeded 3 per cent. of the value of μ the consequent error in X would become sensible.

A change of 30° C. in temperature seldom alters m by more than 2 per cent., so in temperate regions the neglect of temperature variation in μ seems hardly likely to lead to error, unless of course temporary magnetism is more susceptible to temperature changes than permanent magnetism.

In tropical countries the neglect is likely to be more serious.

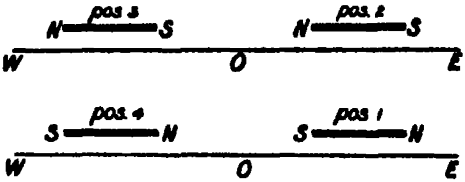
[Since this paper was written, Professor Mascart, in 'Terrestrial Magnetism,' has called attention to a source of uncertainty which I had supposed negligible. He points out that in the vibration experiment the proper induction coefficient is really $\mu - \nu$, where μ has the meaning attached to it here, while ν is the coefficient of induction for a magnetic field perpendicular to the magnet's length. Professor Mascart describes a method of determining experimentally μ, ν, and μ'—the coefficient when temporary and permanent longitudinal magnetisations are opposed. The method is, he says, easily applied and the results consistent; but the apparatus required seems a little complicated. Professor Mascart quotes no numerical results.

In the Kew pattern collimator magnet I should anticipate ν/μ to be small, but it may not be negligible. Experimental investigation of the point is certainly desirable.]

Asymmetry in Magnets.

§ 39. In the deflection experiment it is customary to describe the collimator magnet as being on the *east* or *west* side of the mirror magnet, and as having its north pole *east* or *west*. The directions are not strictly east and west, but perpendicular to the mirror magnet, as deflected out of the magnetic meridian. As the sense intended, however, is quite clear, I shall employ the usual terminology.

In the accompanying figure, which is not drawn to scale, W and E represent the "west" and "east" ends of the graduated bar on which slides the frame carrying the collimator magnet. The graduations run in both directions from O, the centre of the bar. The centre of the mirror magnet should be on the vertical line through O, and also on the horizontal line which is the prolongation of the magnetic axis of the collimator NS. The figure shows the collimator magnet in the four positions which it occupies during the deflections made at one of the two distances, say 30 cm. Supposing a 30 cm deflection to be the first of the day, the four positions are assumed in the order 1, 2, 3, 4. It is the rule at Kew Observatory that if a 30 cm. observation takes precedence on a given occasion, then a 40 cm. observation has precedence on the next On the second occasion the four 30 cm. positions occur in the order 2, 1, 4, 3. The object is very probably to eliminate so far as possible the influence of changes of force or temperature on the value of P, which is calculated from a large number of observations.



§ 40. If there were perfect symmetry one should have identical readings in positions 1 and 4, and again in positions 2 and 3. But, in reality, agreement in these readings is wholly exceptional. To eliminate the disturbing influence of observational errors and changes in force a number of deflection experiments are necessary. Thus, the only case in which I have had the data requisite for an exhaustive study is that of the Kew unifilar itself.

In this instance the phenomena seem adequately explained by the hypothesis of the existence of such asymmetry as we should anticipate *à priori*.

It is doubtless the aim of the maker to fit the suspension tube so that the centre of the mirror magnet and the centre, or zero, of the graduated bar in the deflection experiment shall lie in a vertical plane perpendicular to the bar; but complete success is unlikely to attend his attempts. Accordingly, my first assumption is that the centre of the mirror magnet is really at a distance z, counted positive towards the "east," from the vertical plane perpendicular to the bar through the centre O.

The frame carrying the collimator magnet has a fiducial mark at the centre of its edge, which is set during the deflection experiment at the division 30 or 40 cm. on the bar. From the centre of the upper edge of the frame a pillar extends vertically upwards, so as to fit into the hollow shank which forms part of the appendages of the collimator magnet. The vertical plane perpendicular to the bar through the fiducial mark should contain the centre of the vertical pillar and also that of the magnet. Supposing the frame to have fiducial marks on both sides, and that these agree, one can test the existence of the first possible cause of asymmetry by turning the frame end for end, keeping the direction of the magnet unchanged, the azimuth of the mirror magnet will serve as a criterion This kind of asymmetry has not been detected in the Kew unifilar, and if existent its influence would be nearly eliminated in a large number of observations in which either position of the sliding frame occurred promiscuously. The existence of the second species of asymmetry is less easily tested, but is more probable *à priori*. Accordingly my second assumption is that during the deflection experiments the centre of the magnet is at a distance y from the vertical plane which passes through the fiducial mark, and is perpendicular to the magnet's length. Here y is counted positive when the magnet's centre and north pole are on the same side of the fiducial mark.

§ 41 On these two hypotheses it is obvious from fig. 1, that when the apparent distance between the centres of the magnets is r, the true distances are as follows .—

Position.	1	2	3	4.
True distance . . .	$r+y-z$	$r-y-z$	$r+y+z$	$r-y+z$.

Now suppose that u and $u+\delta u$ are the deflection angles of the mirror magnet answering to the true distances r and $r+\delta r$. Then, according to the first approximation formula,

$$(r+\delta r)^3 \sin(u+\delta u) = r^3 \sin u ,$$

whence, neglecting squares and products of δu and δr, we have

$$\delta u = -3r^{-1} \tan u \, \delta r \quad . \ \dots\dots\dots\dots \ . \ . \quad (10).$$

In what follows I employ u when $r = 30$ cm., and u' when $r = 40$ cm., and distinguish the several positions of fig. 1 by suffixes.

Referring to the scheme of values of δr given above, we find—

Position 1.

$$\delta u_1 = -\tfrac{1}{10}(y-z)\tan u,$$
$$\delta u'_1 = -\tfrac{1}{40}(y-z)\tan u'.$$

Position 2

$$\delta u_2 = \tfrac{1}{10}(y+z)\tan u,$$
$$\delta u'_2 = \tfrac{1}{40}(y+z)\tan u'.$$

Position 3.

$$\delta u_3 = -\tfrac{1}{10}(y+z)\tan u,$$
$$\delta u'_3 = -\tfrac{3}{40}(y+z)\tan u'.$$

Position 4.

$$\delta u_4 = \tfrac{1}{10}(y-z)\tan u,$$
$$\delta u'_4 = \tfrac{3}{40}(y-z)\tan u'.$$

Whence

$$\delta u_4 - \delta u_1 = \tfrac{1}{5}(y - z)\tan u, \qquad \delta u_2 - \delta u_3 = \tfrac{1}{5}(y + z)\tan u,$$

$$\delta u'_4 - \delta u'_1 = \tfrac{1}{20}(y - z)\tan u', \qquad \delta u'_2 - \delta u'_3 = \tfrac{3}{20}(y + z)\tan u'.$$

The mean results from the thirty-nine absolute determinations of horizontal force made with the Kew unifilar in 1892, were as follows:—

$\delta u_4 - \delta u_1$	$\delta u_2 - \delta u_3$	$\delta u'_4 - \delta u'_1$	$\delta u'_2 - \delta u'_3$.
10′ 22″.	27′ 29″.	3′ 1″.	8′ 25″.

Also, approximately,

$$u = 15^\circ\, 30', \qquad u' = 6^\circ\, 30'.$$

Thence we have—

From observations at 30 cm.	From observations at 40 cm.
$y + z = 5 \times 3{\cdot}606 \times 0{\cdot}00799 = 0{\cdot}144$ cm,	$y + z = 6{\cdot}6 \times 8{\cdot}777 \times 0{\cdot}00245 = 0{\cdot}143$ cm,
$y - z = 5 \times 3{\cdot}606 \times 0{\cdot}00302 = 0{\cdot}054$,,	$y - z = 6{\cdot}6 \times 8{\cdot}777 \times 0{\cdot}00088 = 0{\cdot}052$,,
whence	whence
$y = 0{\cdot}099$ cm. $z = 0{\cdot}045$ cm.	$y = 0{\cdot}098$ cm. $z = 0{\cdot}045$ cm.

If the hypotheses on which the calculations are based are incorrect, the agreement between the two sets of values found for y and z is certainly remarkable.

I repeated the calculations for the data from the same unifilar in 1893, and again the two sets of values for y and z were in excellent agreement, the means being

$$y = 0{\cdot}095 \text{ cm.}, \qquad z = 0{\cdot}039 \text{ cm.}$$

§ 42. *Consequences of Asymmetry.*—Using the first approximation formula, we have of course when we neglect δr^2 and δu^2

$$\delta u_1 + \delta u_2 + \delta u_3 + \delta u_4 = 0.$$

Thus to estimate the size of the error we must go as far as squares and products of small quantities, replacing (10) by

$$\delta u = -3r^{-1}\tan u\,\delta r + \tfrac{1}{2}\tan u\,\delta u^2 - 3r^{-1}\delta u\,\delta r - 3r^{-2}\tan u\,\delta r^2 \ldots \quad (11).$$

Substituting the first approximation value for $\delta u/\delta r$ in the small terms in the usual way, we find

$$\delta u = -3r^{-1}\tan u\,\delta r + \tfrac{3}{2}\tan u\,(3\tan^2 u + 4)\,r^{-2}\delta r^2 \ldots\ldots \quad (12).$$

Taking $r = 30$, and ascribing to δr in succession the values corresponding to the four positions, I find on reduction

$$\delta\Pi \equiv \tfrac{1}{4}(\delta u_1 + \delta u_2 + \delta u_3 + \delta u_4) = \tfrac{1}{600}\tan u\,(3\tan^2 u + 4)(y^2 + z^2) \ldots (13).*$$

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* The method followed above has the advantage of leading directly to the value

Taking the data for the Kew unifilar in 1892, we find

$$\delta\bar{u}' = 23 \times 10^{-6} \text{ (or about } 48''\text{)}.$$

Now $\delta\bar{u}$ is the error, in circular measure, in the observed deflection u, and the consequent error in the horizontal force, deduced from the relation

$$X^2 \sin u = \text{constant},^*$$

is

$$\delta X = -\tfrac{1}{2} X \cot u \delta\bar{u} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (14).$$

In the case of the Kew unifilar, we have approximately

$$X = 0{\cdot}183, \qquad \cot u = 3{\cdot}61,$$

whence

$$\delta X = -0{\cdot}0000075 \text{ C.G.S., approx.}$$

Thus the error is nearly one unit in the last significant figure. As the error varies as $y^2 + z^2$, it would become of very sensible magnitude if y and z were twice or thrice as large as in the Kew unifilar. If I may judge from the figures recorded of unifilars under examination at the Observatory, such large values of y and z really exist.

The source of error just considered alters the observed deflections at 30 and 40 cm. in the same sense, and seems unlikely to exert an appreciable influence on the value of P.

§ 43. *Causes of Asymmetry.*—A collimator magnet ought to be adjusted in its stirrup until it is exactly horizontal in the vibration experiment. The magnetic forces acting on it tend, in this magnetic hemisphere, to pull the north pole down, and consequently the centre of gravity of the composite body composed of magnet and appendages must be out of the vertical, which is the prolongation of the suspending fibre, and on the same side of it as the south pole. If the stirrup is symmetrical, and if the lens and scale of the magnet are equal in weight and symmetrically situated, then the C.G. of the magnet itself must, in this hemisphere, lie on the same side of the suspending fibre as the south pole.

Now the suspending fibre in the vibration experiment, if produced, should cut the magnetic axis at the same point as it is cut in the deflection experiment] by the vertical plane which is perpendicular to the magnet, and contains the fiducial mark on the sliding frame.

Thus in a perfectly symmetrical magnet the middle point of the magnet's length, and so the middle point of the line joining the "poles" (assuming magnetic symmetry) must lie on one side of the fiducial mark, and so of the graduation on the bar which coincides with it.

Calling the error thus introduced in the distance r between the

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of $\delta\bar{u}$, a quantity whose physical significance is easily grasped. But a sufficiently exact value of δX can be obtained more simply from the relation $(r + \delta r)^3 (X + \delta X)^2 = r^3 X^2$ (see (4)).

magnets' centres y, measured as in § 40, we can find its value if we know the circumstances of the case. Thus suppose the magnet to have a moment 840 and to weigh, with lens and scale, 30 grams; then at a place where g (gravity) is 980, and the vertical magnetic force is 0·44, we have

$$-y = (840 \times 0{\cdot}44) \div (980 \times 30) = 0{\cdot}013 \text{ cm., approx.}$$

The value thus found for y is not only much less numerically, but even opposite in sign to the value calculated for the Kew collimator magnet in § 41. There *is nothing surprising in this*, because the perfect symmetry which our last calculation assumes in the magnet and its appendages is a remote probability. For instance, the lens and scale of the magnet, in the only case where I had them weighed, differed by nearly half a gram. If other things were symmetrical this alone would remove the C.G. nearly 0 08 cm. from the symmetrical position.

Mechanical asymmetry might arise in many other ways. The magnet itself, for instance, might be slightly conical.

It is also at least conceivable that magnetic asymmetry may exist. If a magnet were conical, or had one end tempered differently from the other, I see no reason to expect its "poles" to be equidistant from the middle point of its length.

§ 44. Whilst numerous causes may contribute to the asymmetrical displacement denoted by y, the first mentioned appears the most interesting. Not merely is it, as we have seen, unavoidable, but it varies if either m or the vertical force alters.

The vertical force on the earth's surface varies from about + 0·6 to − 0 6 C.G.S unit. Thus if the typical magnet above considered were adjusted so as to be horizontal under the one limiting force, it would have to be shifted about 0·034 cm. in its stirrup to become horizontal under the other.

The shifting in the stirrup entailed by this variable cause of asymmetry slightly affects the moment of inertia. For instance if the symmetrical magnet considered above, for which $y = 0{\cdot}013$ cm. at Kew, were used in a tropical station where the vertical force vanished, y should be reduced to 0, and the consequent change in moment of inertia would be

$$\delta K = -30\,(0{\cdot}013)^2 = -0{\cdot}005 \text{ C.G.S. unit, approx.}$$

The mean K in Table I is $2711/\pi^2$, or 274 approximately; and with these values

$$\delta K/K = -2 \times 10^{-5}, \text{ approx.}$$

Supposing no allowance made for this, the consequent error in X would be

$$\delta X = -X \times 10^{-5}.$$

This would hardly be sensible supposing X measured as usual to five significant figures. It might happen however that owing to asymmetry in the stirrup the C G. of the magnet—with scale and lens—had to lie, in the absence of vertical force, at a distance d, comparable with 1 mm. on one side of the suspension fibre. Under such circumstances the change in the moment of inertia when the vertical force altered from 0·44 to 0 would exceed that calculated above in the ratio

$$2d : 0{\cdot}013.$$

As perfect mechanical symmetry must be the exception, we see that this source of uncertainty is likely to be considerably more serious than might be supposed from our first example.

The above considerations may suggest that shifting the collimator magnet in its stirrup is a mistake. If, however, this is not done when required to keep the magnet horizontal, the magnet and appendages must tilt over slightly. This not only alters the moment of inertia, but likewise prevents the magnetic couple in the vibration experiment from having its proper value.

The source of trouble we have just been considering is distinctly exceptional in this respect, that it is most serious in magnets of large magnetic moment.

Law of Action between Magnets.

§ 45 The formula

$$\text{couple} = 2mm'r^{-3}(1 + Pr^{-2} + Qr^{-4} + \ldots\ldots)$$

assumes that the centre of the mirror magnet lies on the prolongation of the axis of the collimator magnet, and that the axes of the two magnets are perpendicular to one another. With the magnets in the position supposed, it is not difficult to calculate the values of P and Q, if we can regard a magnet as consisting of two equal and opposite "poles," the product of whose distance into the strength of either constitutes the magnetic moment Thus if 2λ and $2\lambda'$ be the distances between the poles of the collimator and mirror magnets respectively, I find

$$\text{couple} = 2mm'r^{-3}\left\{1 + (2\lambda^2 - 3\lambda'^2)r^{-2} + \tfrac{3}{8}(8\lambda^4 - 40\lambda^2\lambda'^2 + 15\lambda'^4)r^{-4} + \ldots\ldots\right\} \quad \ldots\ldots \ (15),$$

so that

$$\left.\begin{aligned} P &= 2\lambda^2 - 3\lambda'^2 \\ Q &= 3(8\lambda^4 - 40\lambda^2\lambda'^2 + 15\lambda'^4)/8 \end{aligned}\right\} \quad \ldots\ \ldots\ldots \ (16).$$

When $r = 30$ cm.

$$\left.\begin{aligned} Pr^{-2} &= (2\lambda^2 - 3\lambda'^2)/900 \\ Qr^{-4} &= (8\lambda^4 - 40\lambda^2\lambda'^2 + 15\lambda'^4)\,10^{-4}/216 \end{aligned}\right\} \quad \ldots\ldots\ldots \ (17).$$

We have $P = 0$ when $\lambda'/\lambda = \sqrt{2/3} = 0{\cdot}8165$, approx.,

$Q = 0$ „ $\lambda'/\lambda = 0{\cdot}467$, approx.

We can thus make either P or Q vanish, but not both simultaneously.

When

$$P = 0,$$
$$Q = -9\lambda^4/2.$$

§ 46. Both collimator and mirror magnets are comparatively "short," *i.e.*, their lengths are only about ten times their diameters. In such magnets we should expect, according to Coulomb,

$$2\lambda = 2l/3, \qquad 2\lambda' = 2l'/3,$$

where l and l' are the lengths of the magnets. The formulæ by Green, Jamin, and others are more complicated.

Perhaps all we are justified in saying, *à priori*, is that λ and λ' cannot exceed $l/2$ and $l'/2$, and that the collimator and mirror magnets are sufficiently similar in pattern to make it probable that the assumption

$$\lambda'/\lambda = l'/l$$

is not far wrong

To throw some light on the question, I had measurements made of the magnets of the Kew unifilar and of a unifilar of class E The Kew unifilar belongs to group B, and its P is negative but exceptionally small. The instruments of group E are, as we have already seen, exceptionally uniform in character, and invariably have their P large and positive.

The results were as follows, lengths being in centimetres,

Unifilar.	l.	l'.	l'/l
Kew, group B	9·35	7·60	0·81
„ E	9·17	6·35	0 69

Supposing $\lambda'/\lambda = l'/l$, we should have in these two cases

λ'/λ.	P/λ^2.	Q/λ^4.
0·81	0·00	−4·4
0·69	+0 57	−2·9

These figures, taken in conjunction with our previous data, are on the whole distinctly favourable to the hypothesis

$$2\lambda/l = 2\lambda'/l' = n \text{ (a constant)} \quad \ldots\ldots\ldots \quad \ldots \quad (18)$$

Accepting this hypothesis provisonally, I had the curiosity to see what value we should obtain for n by ascribing to P the mean value found for unifilars of group E, taking for l and l' the values quoted above for a unifilar of that group.

Equating to one another the two values of Pr^{-2} at 30 cm., the one as given above, the other as given in Table I, we have

$$0·57\lambda^2 \times 30^{-2} = 655 \times 10^{-5}.$$

Thus $$\lambda = 3·21, \text{ approx},$$

and so $$n \equiv 2\lambda/l$$
$$= 6·24 \div 9·17 = 0·67, \text{ approx}.$$

The practically exact agreement with Coulomb's relation

$$2\lambda = 2l/3$$

seems to warrant the conclusion that if we employ this relation in calculating Q the result is likely to be of at least the right order of magnitude.

On this hypothesis, taking the values 9·35 and 9·17 for l, as fairly representative of the two groups, we have with $r = 30$ cm.,

Probable value of Qr^{-4} in unifilar of group B $= -5 \times 10^{-4}$, approx.

" " " E $= -3 \times 10^{-4}$ "

Assuming the value of P to be correctly determined, the error in X due to the omission of the Q term is given by

$$\delta X/X = -\tfrac{1}{2}Q/30^4.$$

Taking the numerical values found above for Q, we should have when X = 0·18—

For mean unifilar of group B, $\delta X = +5 \times 10^{-5}$, approx.

" " " E, $\delta X = +3 \times 10^{-5}$, "

There is admittedly much uncertainty in these numerical estimates, but they undoubtedly indicate that the neglect of Q requires justification.

If the Q term is not negligible then the ordinary method, which assumes that the P term is the sole cause of the difference between the two values of m/X, given by the first approximation formula with $r = 30$ and $r = 40$, must lead to a wrong value for P.

Now the P correction does not strike a mean between the results at 30 and 40 cm., but adds to or subtracts from *both* values of m/X. Thus the indirect consequences of neglecting Q may be as important as the direct.

[Lamont, in his 'Handbuch des Erdmagnetismus' (Berlin 1849), discusses the general case of the action of one magnet on another, the distribution of "free" magnetism being arbitrary. The results (16) for

P and Q, though apparently not given explicitly by Lamont, could be easily deduced from his general formulæ, see especially his pp. 44 and 45. Dr. C. Borgen, in 'Terrestrial Magnetism,' October, 1896, pp 176–190, carries Lamont's formulæ somewhat farther. The position taken by the two magnets in the deflection experiment is that termed by Borgen "Erste Lamont'sche Hauptlage." According to Lamont's formulæ, the relative importance of Q, and the values of l'/l, for which P and Q vanish, must vary according to the law of distribution of free magnetism At the same time, it would appear that so long as the law of distribution is the same for the two magnets, the conditions for P vanishing is hardly likely to depart much from

$$l'/l = \sqrt{2/3}.$$

The idea adopted in the text of assuming "poles," and proceeding on the hypothesis

$$2\lambda/l = \text{constant},$$

is one of which Lamont says on his p 45, "Dies ist ein sehr nutzlicher Mittel um approximativ den Werth der hoheren Glieder zu bestimmen ." Thus, though not new, as I had supposed, it has the advantage of being recommended by one of the leading pioneers of terrestrial magnetism.]

§ 47 The exact symmetry in the position of the two magnets which is postulated in obtaining the formula for the deflection is rather to be hoped for than expected. For instance, the magnetic centre of the mirror magnet is hardly likely to lie exactly on the prolongation of the magnetic axis of the collimator magnet, nor is the magnetic axis of the mirror magnet likely to be exactly normal to the mirror I have, however, no experimental data bearing on either of these points.

§ 48. The present paper may seem premature, in view of the number of questions which it raises without finally answering. To wait, however, until experiment had given a final answer to all the questions would simply mean shelving the whole subject indefinitely. Many of the questions could be satisfactorily dealt with only by elaborate experiments. Such experiments are hardly likely to be carried out at any public institution, within a reasonable time, until the necessity for them has been clearly demonstrated.

The investigations embodied in this paper have extended over several years, and the results obtained are likely, I think, to be of immediate use.

The unifilar magnetometer is not the only instrument capable of measuring the horizontal force. Induction coil methods have of late years been introduced and advocated by eminent authorities. The question of the relative merits of magnetometers and induction coils is very likely, I think, to come to the front in a few years, and it is hardly likely to be adequately considered unless the sources of weakness in the respective methods are clearly understood.

Again, there has been an increasing tendency of late years, in most lines of physical research, to add figure to figure, and to aim at higher accuracy, or at least the appearance of higher accuracy. If people were content with giving magnetic force to four significant figures, the majority of the sources of uncertainty specified in this paper need awaken little apprehension. But now-a-days hardly any one is content with less than five significant figures, and one occasionally sees six. I am not prepared to say that the retention of six figures is indefensible in the case of open range magnetographs, when it is clearly understood that *differences* only are concerned, but I do think that with unifilars of the Kew pattern, as hitherto made, the least probable error we can reasonably expect in an absolute measurement is two or three units in the fifth significant figure.

§ 49. We have seen that there is every reason to expect that the values of the horizontal force given by a unifilar are in error to a different extent, according to the temperature and the magnitude of the force. This must introduce a source of uncertainty into the comparisons effected between unifilars at distant observatories through the intermediary of a travelling instrument. To adequately forewarn those engaged in such comparisons may be to put them in a position to obtain more satisfactory results.

§ 50. For the opinions expressed in this paper, and for the accuracy of its conclusions, I am alone responsible; but it is only proper that I should acknowledge the valuable assistance given me by Mr. T. W. Baker, Chief Assistant at the Kew Observatory. For many years Mr. Baker has taken the great majority of magnetic force observations at Kew, and his experience in determining the "constants" of collimator magnets is probably quite unique. Mr. Baker has always been ready to place his great practical knowledge of the subject freely at my disposal, and I have also to thank him for carefully carrying out a variety of special experiments, made to elucidate doubtful points at various stages of the inquiry.

"The Absorption of Rontgen's Rays by Aqueous Solutions of Metallic Salts."

By the Right Honourable Lord Blythswood, LL.D., and E. W. Marchant, D.Sc. Communicated by Lord Kelvin, F.R.S.

Received March 11,—Read June 15, 1899.

The absorption of X rays by metallic salts is a subject that has not received very much attention up to the present time, although it appears to be of considerable importance. It seemed possible that if the absorption were atomic, that is if the absorption of the rays were not produced so much by the nature of the molecule as by the constituents of the molecule, there ought to be some simple relation between the absorbing powers of salts having the same acid radicle. This investigation, however, was originally suggested with a view to finding out whether the very great difference in the transparency of individuals to Rontgen rays might not be caused by an excess of some salt in the composition of the blood or muscular tissue.* Some results have been obtained by different experimenters, the most exhaustive treatment of the subject up to the present having been given by Dr. Gladstone and Mr Hibbert (B.A. Reports, Section B, 1896, 1897, 1898).† They found that the physical condition of the absorbing substance (whether solid or liquid) produced no very marked differences, also that the absorption of a mixture of salts was the same as that of the double salt in the same state of division, and that the change in atomicity of the absorbing substance produced no appreciable effect, also that the total absorption produced by a solution was the sum of the absorptions of the salt and its solvent.

They also found that the absorptive power depended on the atomic weight and not on the density or combining proportions, using an aluminium stepped scale they found that the absorption appeared to increase logarithmically with the thickness of aluminium traversed. The same experimenters have also examined the absorptive power of various metals and metallic salts, and have compared that of different acid radicles. Before, however, their results were published the present investigation was taken up, and the results obtained appear to be of considerable interest Similar results have been obtained by several others,‡ but all are included in the more exhaustive work of Dr. Gladstone and Mr Hibbert.

The great difficulty found in the investigation was the obtaining of quantitative results, as, although it is a comparatively simple matter to obtain approximate qualitative results, it is much more difficult to obtain even approximately accurate quantitative values. The only method which appeared to offer much prospect of success was a photographic one, and all the values given below have been obtained photographically

The first method adopted was to place two cells made of thin glass in front of two holes cut in a thick lead sheet, one cell being filled with water and the other with the solution to be tested, the photographic plate was placed behind the lead sheet, and exposed generally for ten minutes to the rays. The tube was placed in such a position that the anti-cathode was in the normal drawn to the photographic plate from the point midway between the two holes, and was 12 inches distant from it. The plates, after development, were placed in the photometer, and the relative intensities of the two spots compared, the arrangement adopted being that shown below, fig 1, in which the two spots were illuminated by the light from two candles

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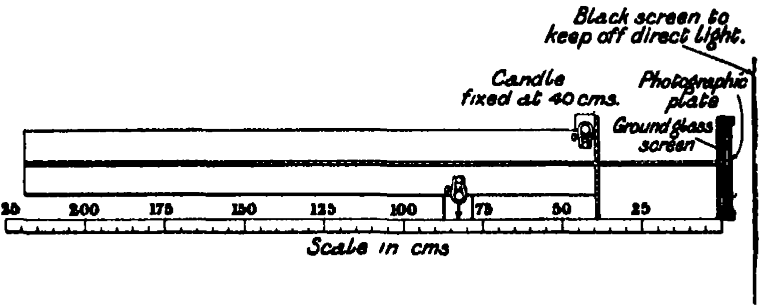
* *Cf.* Bouchard, 'Comptes Rendus,' vol 123, pp. 967 and 1042.

† 'Chemical News,' Nov. 13, 1896, and Oct 21, 1898.

‡ Meslans, 'Comptes Rendus,' vol 122, p. 307; Bleunard and Lablesse, *ibid.*, vol. 122, pp. 527 and 723; Van Aubel, 'Jour de Physique,' vol 5, p 511; Novak and Sulc, 'Beiblätter,' vol. 20, part 5, p. 414; F. Rè, 'Elettricità' (Milan), pp. 261-264 (1898).

Fig 1.



A ground glass screen was placed behind the plate to obtain uniform illumination of the spots. It was found with this arrangement that the relative intensities could be determined with considerable accuracy, but that the relative intensities depended to a certain extent on the time of development, as has also been noticed by Hurter and Driffield. Another disadvantage of this method was that the allowance that had to be made for the absorption of the glass was a comparatively large one with many solutions, and that the error incident on the determination of this correction was such as to materially alter the relative absorbing powers Thus if a be the percentage of absorption due to the glass of the cells (which was within limits of experimental error equal for the two cells employed), b that due to the water in one cell, and $b+c$ that due to the solution in the other cell, c being supposed to be that part of the absorption due to the presence of the dissolved salt, the ratio actually observed in this method by comparing the intensities of the two spots is—

$$\frac{a+b+c}{a+b} = \frac{\text{absorption due to the cell and dissolved salt}}{\text{absorption due to the cell and water}}.$$

If therefore c is small compared with b or a, the accuracy attainable is very much reduced. For very absorbent substances, however, this method gave fairly good results.

A very important question in connection with this investigation was the standard with which all these aqueous solutions were to be compared. It seemed most satisfactory, since all the solutions that were tested were aqueous, to choose water for the standard absorber, and all numbers given are relatively to water.

Now, to determine the relative absorbing power of the salt in solution, compared with the absorbing power of water, we may consider the absorption of the solution to be split up into two parts, one due to the absorption of the water of which the solution is made up, and the other part due to the salt dissolved.

Now, as above, let a = percentage absorption due to cell, b = percentage absorption due to the water, c = percentage absorption due to the salt. Then first comparing the intensities of two spots, as above, one obtained through an empty cell and one through a cell filled with water, we have

$$\frac{a+b}{a} = \mathrm{K}_1 \text{ (say)}, \qquad 1+\frac{b}{a} = \mathrm{K}_1, \qquad \text{and} \qquad a = \frac{b}{\mathrm{K}_1 - 1}$$

Then, comparing the intensities of two spots, one obtained through a cell filled with water, and the other through a cell filled with the solution, we have

$$\frac{a+b+c}{a+b} = \mathrm{K}_2 \text{ (say)},$$

$$c = (a+b)(\mathrm{K}_2 - 1),$$

and substituting for a, we have

$$c = b\,\frac{\mathrm{K}_1(\mathrm{K}_2 - 1)}{\mathrm{K}_1 - 1},$$

$$\frac{c}{b} = \frac{\mathrm{K}_1(\mathrm{K}_2 - 1)}{(\mathrm{K}_1 - 1)}.$$

The assumption here made is that the intensity of the black spot on the negative is proportional to the intensity of the light falling on it.

With the cells used $\mathrm{K}_1 = 1{\cdot}7$.

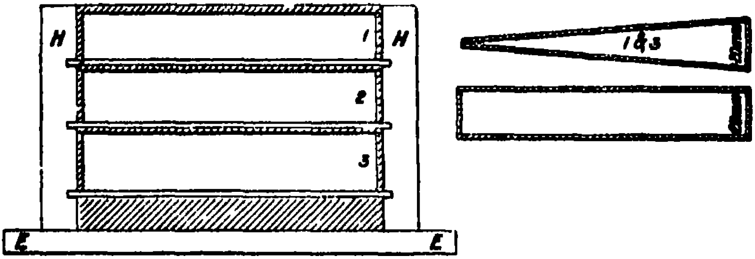
The second method used for the determination of the relative absorbing powers, was based on the fact that the amount of absorption of any substance varies with the thickness of the layer traversed by the ray.

The arrangement of the apparatus was as shown in fig. 2.

EE is a wooden base supporting the two uprights HH, to which is attached a thick lead sheet (shaded diagonally in the figure). In the uprights, HH, three grooves are cut, into which fit the bases of the three cells, 1, 2, 3, which are thus supported one above the other on their wooden stand. The cells 1 and 3 are wedge-shaped, being 137 mm. long, and 20 mm. wide at the broadest part, sloping away to nothing at the thin end of the wedge. The cells are about 25 mm.

deep. The middle cell 2 (above) is oblong in shape, being 137 mm long by 20 mm. wide. All the cells are made of paraffined mahogany about 1·6 mm. thick. On the other side of the lead sheet the dark slide containing the plate is placed, stops being arranged so that the plate is always in the same position relatively to the wedges.

FIG. 2.



Opposite the cells three oblong holes 115 mm long by 20 mm. wide are cut in the lead sheet, so as to allow the rays to fall on the photographic plate; the focus tube used was placed 300 mm distant from the plate, and was arranged so that the centre of the reflector or anticathode was opposite the middle of the centre hole in the lead.

In an experiment the two wedge shaped cells 1 and 3 were filled with the solutions that required testing, while the middle cell 2 was filled with water. It was found that the intensity of the radiation from the tube was very nearly constant over the whole plane of the plate, the variation in intensity of the strip obtained opposite the middle slit varying very little from one side to the other. Also, no difference could be perceived when the cells 1 and 3 were filled with the same liquid, thus showing that the variation in the vertical direction was negligible.

In all experiments the same focus tube was used, the intensity of the X rays being adjusted by heating. To give an idea of the penetrative power of the rays used, it may be stated that at a distance of 12 inches from the tube, the bones of the hand were clearly visible on a fluorescent screen, while the flesh still appeared dark. By this very rough test the tube was brought as nearly as possible to the same condition for each experiment. It was found, however, that the use of different penetrative powers had very little effect on the *relative* absorbing powers of the solutions, though the actual intensity of the rays was greatly altered. In all experiments the plates used were Edwards' "Cathodal Plates" (5 inches by 4 inches); they were developed by hydrokinone and potassium carbonate in from ten to twelve minutes during the warm weather, the time necessary being increased during the colder months.

Now to determine the absorbing powers by measurement of these plates, a special form of photometer was devised for the purpose, as shown below, fig. 3.

Fig. 3.—Plan.

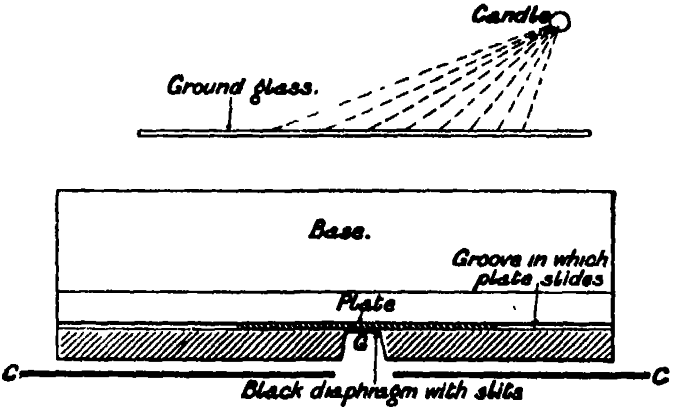
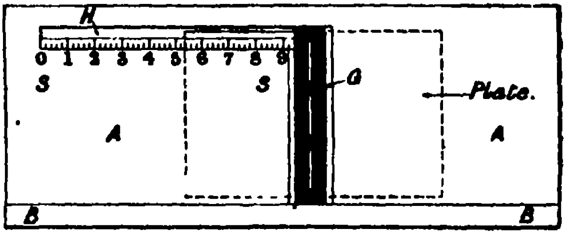


Fig. 4.—Elevation.



AA is a piece of wood about 10 inches by 6 inches, fixed to the base BB, and fitted at the back with grooves, so that a plate can slide from one end to the other along the grooves, figs. 3 and 4. G is a vertical slot cut in the wood, into which a diaphragm is fitted, pierced by three narrow slits, as shown in fig. 4, special precautions being taken that the three slits were all of the same width (0.5 mm.); H is a horizontal slot, cut as shown, to the bevel side of which a scale, SS, is attached. CC, fig 3, is a blackened screen, placed so as to screen the eye from all light, except that which appears at the slits in G.

A piece of ground glass or white paper was placed behind the slit so as to form a uniformly illuminated background.

The plate was shifted along its slide until the intensity of the illumination of the slits 1 and 2 (say) was equal, the reading on the scale opposite the edge of the plate was then noted, a series of values was taken and the mean value calculated. The scale was so placed that the scale reading gave the distance from the end of the strip corresponding to the thin edge of the wedge in the photographing apparatus, in terms of the whole length of the wedge.

Results.

	Weight (in grams) of anhydrous salt in 100 c.c. of normal solution	Thickness (in mm.) of normal solution necessary to produce the same absorption as 20 mm. of water.
Bromides —		
Magnesium	9·2	6·2
Calcium	10·0	5·5
Sodium	10·3	5·0
Iron (Ferrous)	10·8	4·8
Cadmium	13·6	4·1
Potassium	11·9	4·1
Ammonium	9·8	4·1
Zinc	11·3	2·6

These results were obtained by method 2. The absorption both of these and of the iodides is rather too high to make great accuracy possible.

	Weight (in grams) of anhydrous salt in 100 c.c. of normal solution.	Thickness (in mm.) of normal solution necessary to produce the same absorption as 20 mm. of water.
Chlorides —		
Lithium	4·3	18·1
Sodium	5·9	18·0
Potassium	7·5	17·8
Ammonium	5·3	15·1
Magnesium	4·8	13·4
Calcium	5·5	11·2
Cobalt	6·5	9·0
Iron (Ferric)	5·4	8·3
Nickel	6·5	8·2
Zinc	6·8	8·2
Copper	6·7	7·2
Strontium	7·9	6·3
Barium	10·1	5·4
Cadmium	9·1	5·3
Lead	13·9	2·6
Iodides —		
Magnesium	13·8	3·6
Calcium	14·6	3·4
Sodium	14·9	3·1
Zinc	15·9	2·9
Ammonium	14·4	2·9
Potassium	16·5	2·8
Cadmium	18·3	1·9

	Weight (in grams) of anhydrous salt in 100 c.c. of normal solution.	Thickness (in mm.) of normal solution necessary to produce the same absorption as 20 mm. of water.
Acetates :—		
Aluminium (powder)...	6·8	16·0
Iron (powder).........	8·7	10·9
Calcium	7·9	10·8
Potassium	9·8	9·4
Barium..........	12·7	8·2
Copper	9·0	7·0
Lead..................	16·2	2·0
Nitrates :—		
Aluminium	7·1	17·0
Magnesium	7·4	15·2
Calcium...............	8·2	12·4
Potassium.......... ..	10·1	10·8
Cobalt................	9·1	10·6
Nickel................	9·1	8·3
Zinc.................	9·4	7·5
Copper	9·8	7·3
Barium................	13·0	6·7
Strontium*...........	10·6	5·8
Cadmium	11·8	5·5
Lead	16·5	3·1
Uranium..............	14·2	1·9
Sulphates :—		
Aluminium............	5·7	16·0
Magnesium............	6·0	15·2
Sodium...............	7·1	13·5
Ammonium	6·6	13·0
Cobalt	7·7	8·9
Iron (Ferrous).........	7·6	8·0
Copper	7·9	7·1
Nickel...........	7·7	7·0
Zinc..................	8·0	6·8
Cadmium	10·4	4·6
Carbonates :—		
Ammonium	4·8	18·0
Sodium...............	5·3	18·2
Bichromates :—		
Ammonium	12·6	7·3
Potassium (saturated solution)............	..	8·3
Potassium chromate	9·7	7·8

Now proceeding to a consideration of the results obtained, it is at once clear that the absorption, in almost every case, in these normal solutions, increases with the atomic weight, *i.e.*, the higher the atomic weight of the elements of which a salt is composed the greater is the absorption. This effect is most noticeable in the case of bromides and iodides.* In all cases these salts are absorbent, even when the bromine and iodine are combined with the transparent sodium and magnesium. The iodides also are more absorbent than the bromides.

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* This salt contained a good deal of impurity, and the value given is therefore doubtful.

Now proceeding to the consideration of the absorption produced by salts with other acid radicles, the differences are not so marked, but in all cases the least absorbent salt is the nitrate, the next the chloride, the most absorbent being the sulphate, this does not agree with the order of increase of the molecular densities, or of the atomic weights of the constituents. It would therefore appear that the absorptive power is not entirely dependent on the atomic weight of the components of the acid radicle, or on its molecular density; the order in the first case being (1) nitrates ($N = 14$), (2) sulphates ($S = 32$), and (3) chlorides ($Cl = 35{\cdot}5$), (2) and (3) being nearly equal. While in the second place the order should be: (1) Chlorides ($Cl = 35{\cdot}5$), (2) sulphates ($SO_4 = 48$) (3) nitrates ($NO_3 = 62$), while the order actually observed is (1) nitrates, (2) chlorides, (3) sulphates.

Acetates are slightly less absorbent than chlorides, though the number of salts tried is small; only four giving results to which weight can be attached, since aluminium and iron were simply powders in suspension and partially precipitated.

It now remains to be seen if there is any definite connection between the equivalent weight of the bases composing the different salts and their absorptive powers. Their order may be written down as follows:—

In ascending order of absorption:—

1.	Lithium	(7)′	9.	⎧ Nickel	(58·6)″
2.	⎧ Magnesium	(24)″	10	⎨ Copper	(63)″
3.	⎨ Aluminium	(27)‴	11.	⎩ Zinc	(65)″
4.	⎩ Sodium	(23)′	12.	Strontium	(87)″
5.	⎧ Calcium	(40)″	13.	Barium	(137)″
6.	⎩ Potassium	(39)′	14.	Cadmium	(111·7)″
7.	⎧ Iron	(56)″	15.	Lead	(206)″
8.	⎩ Cobalt	(58·6)″	16.	Uranium	(240)‴

The metals bracketed together have very nearly the same absorptive power. The order given above does not agree very nearly with the order of increase of equivalent weight; it agrees more closely, however, with the order of increase of atomic weight. It is to be noticed particularly, that the salts of the alkali metals are less absorbent than might have been expected from their equivalent weight, also that in the calcium, strontium and barium group the amount of absorption does not increase rapidly with the increase in atomic weight. It may therefore be generally stated that the absorptive power of salts having different bases, but the same acid radicle, increases with the atomic weight of the metal forming the base, though not according to any definite law.

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* E. Sehrwald, 'Beiblätter' (1897), p. 64.

This agrees with the results obtained by Dr. Gladstone and Mr. Hibbert.

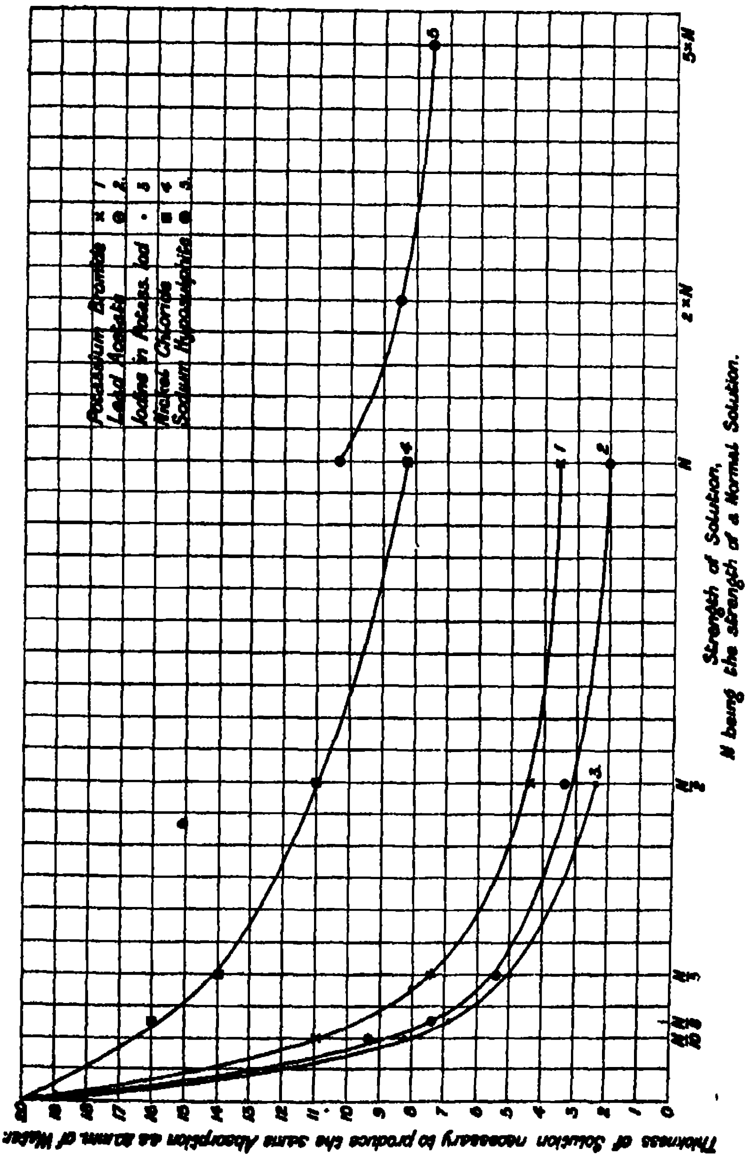
Effect of Rays of different Penetrative Powers.

As stated above, the penetrative power of the X rays used did not have much effect on the apparent relative absorptive powers of the different solutions, but as a still further check on the power of the rays used, the time taken for the developer to bring the strip on the plate photographed, through 20 mm. of water, to sufficient density (allowance being made for the temperature) was always observed, and if this differed much from that necessary on a normal plate, the experiment was repeated. Measurements made on these rejected plates, however, showed that within limits of experimental error, there was no difference between absorptions thus obtained and those calculated from measurements made on fully exposed negatives. Measurements were also made using rays of different penetrative power, and developing for the same time; but the plates obtained admitted of only such rough measurement that here again no differences were detected, the strips being either too dense, or the differences in intensity at the two ends corresponding to the thick and thin ends of the wedges being too slight, to admit of accurate photometry. It may be stated generally therefore that the rays of different penetrative powers do not appear to have any effect on the *relative* absorptive powers of the different solutions.

Effect of Strength of Solution.

In order to determine the effect produced by using solutions of different strengths, a number of measurements were made on various salts: (1) Lead acetate, (2) potassium bromide, (3) iodine in potassium iodide, (4) nickel chloride, (5) sodium thiosulphate (strong solution).

Curves are plotted below showing the relation between the amount of salt in solution and the thickness necessary to produce the same amount of absorption as 20 mm. of water. From these numbers it is clear that the amount of absorption is not proportional to the number of molecules of the salt traversed by the ray, and does not appear to follow any simple law. The connection appears to be logarithmic from the shape of the curves.


Potassium Bromide × 1
Lead Acetate ● 2
Iodine in Potass. Iod · 3
Nickel Chloride ■ 4
Sodium Hyposulphite ● 5
1
2
3
4
5
Thickness of Solution necessary to produce the same Absorption as 20 mm. of Water.
0
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
17
18
19
20
N/10
N/8
N/5
N/2
N
2×N
5×N
Strength of Solution,
N being the strength of a Normal Solution.

Thickness of Solution necessary to produce the same Absorption as 20 mm. of Water.

Strength of solution.	Potassium bromide	Lead acetate.	Iodide in potassium iodide.	Nickel chloride.
N	3·6	2·0	—	8·2
N/2	4·4	3·4	2·4	11·0
N/6	7·4	5·4	5·0	14·0
N/8	—	7·4	6·9	16·0
N/10	11·0	9·3	8·3	—

	Sodium thiosulphate
N	10·6
2 × N	8·5
5 × N	7·5

Effect of Thickness of Solution traversed.

Various experiments have been made in order to verify the statement made by Messrs. Gladstone and Hibbert and others* that the amount of absorption varies logarithmically with the thickness traversed by the ray—

$$r = \log(\lambda t + \mu),$$

where r = percentage of the rays absorbed by the solution under consideration. The results obtained appear to confirm their conclusion. As a preliminary experiment, the intensities at different distances along the strips on three of the plates above considered were carefully compared with the intensity of the strip photographed through 20 mm. of water. Now proceeding to consider these results—

Let A_0 be the original intensity of the X rays.
A_1 the amount of the rays absorbed by the water.
A_2 the amount of the rays absorbed by a given thickness of the solution.
λ_1 be the coefficient of absorption for water.
λ_2 be the coefficient of absorption for the solution.

Then the intensity of the strip obtained by passing the rays through 20 mm. of water is proportional to $A - A_1$, and the intensity of the photographic image at the distance along the strip corresponding to the given thickness of the solution to $A - A_2$, and for the relative intensities of the photographic images (K_2), we have

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* A. Buguet, 'Comptes Rendus,' vol. 125, pp. 398—400; F. Ré, *loc cit.*

$$K_2 = \frac{A - A_2}{A - A_1},$$

or, according to the above assumption,

$$K_2 = \frac{1 - \log(\lambda_2 t_2 + \mu_2)}{1 - \log(\lambda_1 t_1 + \mu_1)}$$

If $K_3 K_4$, &c., are the values of K corresponding to the different thicknesses t_3, t_4, &c., of the solution, we have

$$K_2\{1 - \log(\lambda_1 t_1 + \mu_1)\} = 1 - \log(\lambda_2 t_2 + \mu_2),$$

$$K_3\{1 - \log(\lambda_1 t_1 + \mu_1)\} = 1 - \log(\lambda_2 t_3 + \mu_2).$$

$1 - \log(\lambda_1 t_1 + \mu_1)$ is a constant for any one plate = C (say), hence, subtracting,

$$C(K_2 - K_3) = \log\frac{(\lambda_2 t_3 + \mu_2)}{(\lambda_2 t_2 + \mu_2)}.$$

The value of μ however (being that part of the absorption produced by the walls of the cell and the air traversed) is known to be very small compared with λt, and may therefore in a first approximation be neglected. The above equation then reduces to

$$C(K_2 - K_3) = \log\frac{t_3}{t_2} = \log t_3 - \log t_2.$$

Below are given the results for the three plates used in the test experiments The relative intensities were obtained by means of an arrangement similar to that described for the determination of the relative intensities of the spots on the photographic plate in method I; but arrangements were made to bring the illuminated images nearer to each other to enable more accurate comparison to be made.

The errors, when all precautions were taken, were not greater than 6 per cent. for the determination of the relative intensities, corresponding to 3 per cent. on the determination of the distances of the source of light necessary to produce equal illumination

In the following table—

Column 1 gives the thickness of the solution traversed by the X rays in millimetres.

" 2 gives the logarithms of these thicknesses, to base 10.

" 3 gives the differences of the logarithms.

" 4 gives the intensity of the image photographed through the thickness of the solution given in column 1, in terms of the intensity of the image photographed through 20 mm. of water.

" 5 gives the differences of these intensities.

" 6 gives the ratio of the differences of the logarithms in column 3 to the differences of the intensities in column 5

It is clear therefore that if the logarithmic law holds, the numbers in column 6 should be equal for any given plate.

Nickel Chloride N/8 *Solution.*

t in mm	log.	Diff.	K.	Diff.	Ratio
3·3	0·519	0·18	2·9	0·5	0·36
5·0	0·699	0·225	2·4	0·6	0·37
8·4	0·924	0·28	1·8	0·8	0·35
16·0	1·204		1·0		

The value of C therefore $= 0·36$.

And since $C = 1 - \log(\lambda_1 t_1)$ $\log \lambda_1 t_1 = 0·64$

and since $T = 20$ mm. $\lambda_1 = 0·22$ (for water).

Potassium Bromide N/10 *Solution.*

t in mm.	log	Diff	K.	Diff.	Ratio.
3 3	0·519	0 18	2·4	0·48	0·37
5 0	0 699	0·225	1·92	0·56	0·40
8·4	0·924	0 128	1·36	0·36	0·36
11·0	1·042		1·0		

The value of $C = 0·38$.

And since $C = 1 - \log(\lambda_1 t_1)$ $\log(\lambda_1 t_1) = 0·62$.

and since $T = 20$ mm. $\lambda_1 = 0·21$.

Sodium Thiosulphate, 2XN.

t in mm	log	Diff.	K.	Diff.	Ratio.
3·3	0 519	0 18	2·85	0 85	0·47
5 0	0 699	0·127	2·0	0 56	0·44
6 7	0·826	0·103	1·44	0 44	0·43
8 5	0·929		1 0		

The value of $C = 0·45$.

And since as above $C = 1 - \log(\lambda_1 t_1)$ $\log(\lambda_1 t_1) = 0·55$.

whence (as above) $\lambda_1 = 0·18$.

It will be seen, therefore, that the value of λ was not constant for water, and hence not for solutions, if they obey the same laws. This is confirmed by observations made on plates where the penetrative power was not sufficiently great to give a dense photograph. The variation in the intensity of the photographic image, according to the thickness of the solution traversed, appearing to be much less than with rays of greater penetrative power, although, as stated above, the thickness of the solution necessary to produce the same amount of absorption as 20 mm. of water was constant within limits of experimental error.* This agrees with the observation of M. Buguet (*loc. cit.*). The values of λ for the different solutions have not therefore been calculated.

The value of μ for the apparatus used would (from the comparatively close agreement with theory of the above numbers) seem to be negligible.

The results given above show that the logarithmic law is at any rate approximately true.

Conclusions.

1. The absorption produced by normal aqueous solutions of metallic salts having the same acid radicle, increases with increase of atomic weight of the base.
2. Metals belonging to the alkali group are not very absorbent, neither are those belonging to the calcium, strontium, and barium group, their atomic weight being taken into consideration.
3. Bromides and iodides of the metals are all highly absorbent.
4. With the three common acids, the order of increasing absorptive power is nitrate, chloride, sulphate.
5. The absorption produced by a salt is dependent mainly on the atomic weight of its constituents.

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* Considering this statement in the light of the known facts with regard to the photography of the bones of the hand, it will be seen that facts are not in contradiction to the conclusions drawn from these experiments. With rays of low penetrative power, the whole hand appears dark though the screen appears light (assuming λ large for the flesh and much larger for the bones), the reason being that both the flesh and bones absorb nearly all the rays. Assume, for example, that for this penetrative power the flesh allows only 1/100 of the light to pass and the bones 1/1000. Now, using rays of greater penetrative power we may assume that the flesh will allow 1/10th of the light to pass while the bones will only allow 1/100th to pass. The bones will now be faintly visible. Pushing the penetrative power still further, assume that the flesh now allows half of the light to pass, the bones will allow only 1/20th, and will be very sharply defined With still further exhaustion of the tube the flesh may now be assumed to allow 9/10ths of the light to pass, while the bones still only allow 1/11th; and, as is known, at this stage the bones themselves begin to appear transparent.

At the same time it is not assumed that the ratio of the absorption for the different salts *does* remain constant throughout this very wide range, but that the fact that the ratio of the absorptions appears to remain constant through a limited range while the actual absorption may differ appreciably through this range, is not in contradiction to other observations.

6. The amount of absorption produced by a given thickness of a solution of a metallic salt is not proportional to the amount of salt in solution, but appears to follow approximately a logarithmic law.
7. The amount of absorption varies logarithmically with the thickness of the solution traversed by the rays. The percentage absorption may be represented by an equation of the form $r = \log(\lambda t + \mu)$ where λ is a constant depending on the nature of the solution, and on the penetrative power of the X rays, and t is the thickness of the solution traversed.

It is to be noticed that all the above tables and conclusions are based on measurements made with solutions containing the equivalent weight of the salts, in grams per litre, so that the molecular absorption of the salts with divalent or trivalent bases is greater than that indicated in the tables.

The conclusions 1, 5, and 7, given above, are in exact agreement with those stated by Dr Gladstone and Mr Hibbert in their articles in the 'Chemical News' (*loc. cit.*).

Numerous references have been made throughout this paper to their articles and reports before the British Association, though it was not until the above investigations were nearly completed that attention was drawn to the work that had been done by them.

"On the Resistance to Torsion of certain Forms of Shafting, with special Reference to the Effect of Keyways."

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(Abstract.)

The object of the present paper is to obtain solutions of the problem of Torsion for certain cylinders, whose cross-sections are bounded by confocal conics. It is mainly an extension of de Saint-Venant's investigations, and is based upon his general equations of torsion.

The method employed depends upon the use of conjugate functions ξ and η, such that ξ = const. represents confocal ellipses and η = const. confocal hyperbolas.

The use of conjugate functions for the torsion problem has been suggested by Thomson and Tait,[*] by Clebsch,[†] and by Boussinesq.[‡]

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[*] 'Natural Philosophy.'

[†] 'Theorie der Elasticität fester Körper,' §§ 33—35.

[‡] 'Journal de Mathématiques,' pp. 177—186, série 3, vol. 6.

Clebsch has used such elliptic co-ordinates to solve the torsion problem for hollow cylinders bounded by confocal ellipses, and de Saint-Venant has applied conjugate functions to the same problem for shafts whose sections are sectors of circles; curvilinear co-ordinates have also been employed by Mr. H. M. Macdonald,* but I am not aware that the actual solution has yet been obtained for sections bounded by both ellipses and hyperbolas.

The work proceeds on lines analogous to those developed by Saint-Venant himself in his solution of the problem of torsion for the cylinder of rectangular cross-section. The strains and stresses are expressible in terms of infinite series involving circular and hyperbolic functions.

The boundaries of the section are given by constant values of ξ and η. The values of ξ are taken to be $\pm\alpha$.

The conditions from which the unknown quantity w (the shift parallel to the axis) is determined are

$$d^2w/dx^2 + d^2w/dy^2 = 0$$

throughout the sections; and

$$dw/dn + (mx - ly)\tau = 0$$

along the boundary, where dn = an element of the outwards normal to the boundary, τ is the angle of torsion per unit length, and l, m are the direction-cosines of dn.

Now in the present case

$$dn = \pm d\xi \times (c\sqrt{\mathrm{J}})$$

where $\mathrm{J} = \partial\left(\frac{x}{c}, \frac{y}{c}\right)\Big/\partial(\xi, \eta,)$ at the boundary where ξ = const. and

$$dn = \pm d\eta \times (c\sqrt{\mathrm{J}})$$

at the boundary where η = const., the sign being determined so that dn is positive.

By adding suitable terms to w, we can reduce one or other of the boundary conditions to the form

$$dw_1/dn = 0$$

where $w = w_1$ + suitable terms.

Suppose we make

$$dw_1/d\xi = 0\,;\ \xi = \pm\alpha.$$

Expanding now w_1 in the form of a series,

$$w_1 = \mathop{}\limits_{n=0}^{n=\infty} \mathrm{A}_n \sin h\left\{\frac{m+1}{2\alpha}\pi(\eta+\kappa)\right\} \sin\frac{\overline{m+1}\,\pi\xi}{2\alpha},$$

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* "On the Torsional Strength of a Hollow Shaft," 'Proc. Camb. Phil. Soc.,' vol. 8, 1893, pp. 62 *et seq.*

the differential equation and the first boundary condition are identically satisfied.

When this value is substituted in the second boundary condition, we get an equation expressing a given function of ξ in a series of sines of odd multiples of $\pi\xi/2\alpha$, between the limits $+\alpha$ and $-\alpha$.

But such an expression can be definitely obtained by a method analogous to that for Fourier's series. Comparing coefficients, we obtain relations which determine completely all the constants in the expression of v_1.

w is then known. The shears and torsion moment are then deduced by differentiation and a double integration.

The cross-sections which are dealt with in the present paper, are of very great generality, and they include as special cases many of the cross-sections which Saint-Venant has worked out, for instance the rectangle and the sector of a circle.

The first section of which I treat is that bounded by an ellipse and two confocal hyperbolas. Although the analysis is worked out for the case where the two hyperbolic segments are not symmetrical, I have not given any numerical examples of this case, as the sections obtained by taking two hyperbolas curved the same way do not correspond to any interesting practical case: the section is too broad at the ends and too narrow at the bend, to be any fair representation of the angle iron.

The section bounded by an ellipse and the two branches of a confocal hyperbola is, on the other hand, an approximate representation of a well-known section, much used in engineering practice, the rail section. This section I have worked out for various values of the eccentricity of the ellipse, and of the angle between the asymptotes of the hyperbola. The four sections where this angle is 120° give the best representation of the rail section.

The numerical results are tabulated so as to show the ratio of the torsional rigidity of this section to that of the circular section of the same area, and also the same ratio for the maximum stress.

The ratio of these two ratios gives us a kind of measure of the usefulness or "efficiency" of the section.

In the case of the latter class of sections, I have investigated at length the position of the *fail-points*, or points of maximum strain and stress, the maximum strain, in the case of torsion, being coincident with the maximum stress. It is found that for the two smaller ellipses the maximum stress occurs at the point B, where the section is thinnest. For the two larger ellipses, the maximum stress occurs at four points, F, F, F, F, symmetrically distributed round the contour, and lying on the broad sides of the section. The critical section, when these two cases pass into one another, can be calculated, and is shown in the paper.

The changes in the stresses are shown by curves accompanying the paper, in which the abscissa represents the quantity α, whose hyperbolic cosine and sine are proportional to the major and minor axes of the ellipse respectively, and in which the ordinates represent the stresses at A, B, F, divided by the maximum stress of the circular section of equal area. The curves are, in certain parts, only roughly drawn, but they suffice to show the manner in which the stresses vary. It is seen that the stress at B separates from the maximum stress after the critical value $\alpha = 1 \cdot 225$, and gradually diminishes, compared with stresses at A and F.

This result might have been expected from the investigations of de Saint-Venant upon certain sections bounded by curves of the fourth degree.

These investigations appear, however, not to have been sufficiently noticed. Thomson and Tait in their 'Natural Philosophy,' and Boussinesq in his researches on Torsion,* both conclude that the fail-points are at the points of the cross-section nearest to the centre, and Boussinesq even gives an apparently general proof of this proposition His proof, however, is subject to certain restrictions, which I point out, and which prevent it from being applied to the sections I am dealing with.

The sections are sensibly less useful than the circular section, their torsional rigidity being always diminished and the maximum stress very often increased. This remark, I may add, applies to all the sections dealt with in this paper.

This usefulness or efficiency decreases as the neck of the section becomes more narrow, as indeed might have been anticipated

Other sections worked out are those corresponding to angles between the asymptotes of 90°, 60°, and 0°, in the latter case the sections degenerate into ordinary elliptic sections, with two straight slits or indefinitely thin keyways, cut into them along the major axis, as far as the foci. The stress at the foci, however, is then theoretically infinite

It is interesting to see how, as we make the bend round the foci sharper, the values of α, for which the two fail-points break up into four, become larger and larger, until, when the angle between the asymptotes of the hyperbolas is less than 73°, the greatest stress always occurs at the neck of the section

The limiting case of such sections, when the angle between the asymptotes is very small, and the eccentricity of the ellipse nearly unity, the distance between the foci being very great, gives us the rectangle.

I then pass on to the section bounded by one ellipse and one confocal hyperbola. In the limiting case, when the foci coincide, we obtain the sector of a circle. Of this I have worked out numerically three cases, in each case taking two ellipses.

1. The semi-ellipse.

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* 'Journal de Mathématiques,' série 2, vol 16, p. 200.

2. The ellipse with a keyway cut into it of the shape of a rectangular confocal hyperbola.
3. The ellipse with a single slit cut into it.

The most striking of the results is in reference to the reduction of the torsional rigidity of the ellipse in case (3). This reduction of rigidity decreases rapidly as the depth of the notch decreases.

The rigidity, which is reduced by as much as 23 per cent. when the depth of the keyway is as great as 0 6 (semi-major axis), falls to about 1 per cent when this depth is 0·12 (semi major axis)

Possibly this may throw some light on the fact that the effect of cutting such slits into the material does not always give in practice the reduction in the torsional rigidity which should have been expected from Saint-Venant's results for the circle. Clearly the depth of the keyway is a factor of the very first importance, and keyways of moderate depth will produce a comparatively small effect on the torsional rigidity.

It is also shown that the effect of cutting two equal and opposite slits is practically equal, in the two cases which I have calculated (namely $\alpha = \pi/6$ and $\alpha = \pi/2$), to twice the effect of a single slit.

It seems, therefore, that the study of these sections brings to light several interesting facts in the theory of elasticity, and will well repay the trouble involved in dealing with the long and somewhat tedious algebra and arithmetic which lead to these results.

November 16, 1899.

The LORD LISTER, F.R.C.S., D.C L., President, in the Chair.

Professor W. F. Barrett and Mr. J. S. Gamble were admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

Professor Meldola, Professor Perry, and Dr. R. H. Scott were by ballot elected Auditors of the Treasurer's accounts on the part of the Society.

The following Papers published during the recess, in full or in abstract, in accordance with the Standing Orders of Council, were read in title :—

Sir J. B. LAWES, F.R.S., and Sir J. H. GILBERT, F.R.S. Agricultural, Botanical, and Chemical Results of Experiments on the Mixed Herbage of Permanent Grass-land, conducted for many Years in succession on the same Land. Part III.—The Chemical Results.

Dr. WALLIS BUDGE. On the Orientation of the Temples and Pyramids in the Sûdân. Communicated by Sir NORMAN LOCKYER, F.R.S.

Dr. H. M. VERNON. The Effect of Staleness of the Sexual Cells on the Development of Echinoids.

W. EDMUNDS. Effects of Thyroid Feeding on Monkeys.

Sir W. THISELTON-DYER, F.R.S. On the Influence of the Temperature of Liquid Hydrogen on the Germinative Power of Seeds.

The following Papers were read:—

I. "Note on the Electromotive Force of the Organ Shock, and on the Electrical Resistance of the Organ, in *Malapterurus electricus*." By Professor F. GOTCH, F.R.S., and G. J. BURCH.

II. "On the Formation of the Pelvic Plexus, with especial Reference to the Nervus Collector, in the Genus *Mustelus*." By R. C. PUNNETT. Communicated by Dr. GADOW, F.R.S.

III. "On the Least Potential Difference required to produce Discharge through various Gases." By the Hon. R. J. STRUTT. Communicated by LORD RAYLEIGH, F.R.S.

IV. "Mathematical Contributions to the Theory of Evolution. VII.—On certain Formulæ in the Theory of Correlation, and their Application to the Inheritance of Characters not capable of Quantitative Measurement." By Professor KARL PEARSON, F.R.S.

V. "On the Propagation of Earthquake Motion to Great Distances." By R. D. OLDHAM. Communicated by Sir R. S. BALL, F.R.S.

VI. "An Experimental Research on some Standards of Light." By J. E. PETAVEL. Communicated by LORD RAYLEIGH, F.R.S.

"Note on the Electromotive Force of the Organ Shock and the Electrical Resistance of the Organ in *Malapterurus electricus*."

By FRANCIS GOTCH, M.A., F.R.S, and G. J. BURCH, M.A. Oxon. Received July 18,—Read November 16, 1899

(From the Physiological Laboratory, Oxford)

Electrometer Records of the Organ Shock.

In the month of March of the present year, 1899, the Committee of the Corporation Museum, Liverpool, kindly placed at the disposal of one of us (F. G.) two living specimens of *Malapterurus electricus.* They were exhibited at a lecture delivered in London at the Royal Institution on March 17. One of the fish died from accidental injury, the other was employed by us for the purpose of determining the electromotive force of the organ shock, and for investigating such other points of interest as were feasible

We decided to employ for the determinations the special capillary electrometer made by one of us (G. J. B.), which we had utilised in our researches upon the electrical phenomena of nerve * Our previous work upon the organ of *Malapterurus* rendered it certain that considerable precautions would have to be adopted in connection with this instrument, since it was a much more sensitive one than those we had used in the researches described in our previous paper upon the subject † It was therefore first necessary to make a number of preliminary experiments in order to reduce the extent of any excursion due to the organ shock within such limits that it could be recorded. Several trials with different voltages resulted in our employing a non-inductive shunt of three incandescent lamps; these were placed in parallel between the electrometer connections and had a combined resistance of 26·6 ohms. We diminished the size of the record still further by replacing the high objective of the projecting microscope by a lower power, Zeiss, B.

Our previous experiments made in 1895 showed us that the organ shock developed so rapidly that the records then obtained were all too steep to admit of accurate analysis. It seemed certain, therefore, that the recorded curves, due to the rise and fall of the meniscus, would have to be of a more prolonged type in order that the rate of development of the electromotive force of the shock might be deduced from their analysis.

In the hope of obtaining curves of a sufficiently prolonged character, both the physical and the physiological conditions of experiment were modified; the rate of transit of the photographic plate upon which the image of the mercurial meniscus was projected, was made much quicker, whilst the whole organ of the fish was effectually cooled to a low temperature, 5° C.

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* 'Roy. Soc. Proc.,' vol. 63, p. 300, 1898.

† 'Phil. Trans.,' B, vol. 187 (1896), p. 347

As we had only one fish, we made several attempts to catch a record of the natural discharge of the organ upon the travelling plate. It was found, however, to be practically impossible to do this, since the reflex responses obtained from the uninjured fish were not merely uncertain as to their time of commencement but also very variable as to their intensity.

We determined, therefore, to kill the fish and utilise the nerve organ preparation for the purpose of obtaining the necessary data.

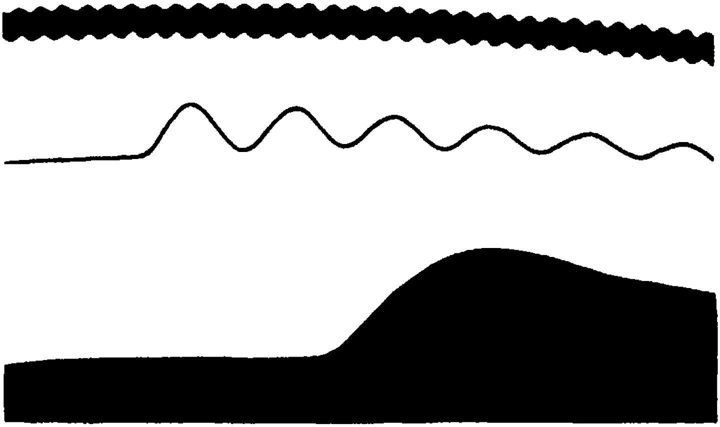
A further consideration induced us to take this course. Determinations made upon an entire fish must be of little value for the calculation of the E.M.F. of the change produced in each excited disc, owing to the complicated physical conditions of an experiment made under these circumstances. On the other hand it appeared to be easy to cut an organ strip and arrange it so that the distance between the contacts, which connected it with the electrometer, should be perfectly definite; moreover, with such a strip the number of discs comprised in the actual distance between the contacts could be enumerated after the experiment by examining appropriate sections made through the whole of this portion of the organ.

The fish was therefore anæsthetised by immersion in ice-cold water and then killed. A strip of the organ was now detached and the nerve carefully dissected out; this nerve organ preparation was placed upon a glass plate in a special moist chamber kept at a uniform temperature of 5° C. The nerve was excited by a single break induced current of such an intensity as our previous experience had shown to be necessary in order to evoke a maximal organ response (Kronecker coil with core, one Daniell in primary, secondary at 10,000). The break induction shock was produced by the movement of the recording pendulum, and was so timed as to occur when the photographic plate carried by the pendulum reached the slit upon which the image of the meniscus was projected. The electrometer contacts were so placed as to lie 15 mm. apart upon the thickest portion of the organ. Thirteen photographs were taken, of which two (Nos. 3012, 3013) gave excellent records suitable for accurate analysis. In the first of these (3012), a facsimile reproduction of which is shown in fig. 1, the three-lamp shunt was placed between the electrometer terminals. In the second record (3013) the two-lamp shunt was employed.

The response in both these experiments was very marked, and, owing probably to the low temperature, was more delayed in its onset and slower in its development than we had anticipated.

After comparatively few successful experiments the excitability of the nerve suddenly failed, and it is interesting to note that this failure was attended by inability of the organ to respond when a stimulus was applied either to the nerve or to the organ substance. It has been recently pointed out by Garton that if the electrical nerves of *Torpedo* are divided in the living fish, and the fish examined nineteen days afterwards, by which time degeneration of the peripheral portion of the nerve has occurred, no response can be obtained from the organ by any stimulus whether applied to the nerve or directly to the organ substance.* These results and the failure referred to in the present instance support the view put forward in our previous paper, that the only excitable structures in the organ are the nerve endings, the organ discs apart from these nerves being inexcitable.†

FIG 1



The upward curve in the lowest line is a record (No. 3012) of the single shock of 15 mm. of electrical organ evoked by a single excitation of the nerve. The curve is to be read from left to right, the moment of excitation being indicated by the commencement of the larger vibrations on the fine middle line. The rate of movement is shown by the tuning-fork record on the upper line, each complete oscillation of which is 0·002" The electrometer terminals were connected by a resistance of 26·6 ohms, which thus shunted a large proportion of the organ effect

The Analysis of the Photographic Records.

The methods used for obtaining data for the analysis of the records differed somewhat from those employed in our previous experiments upon nerves.

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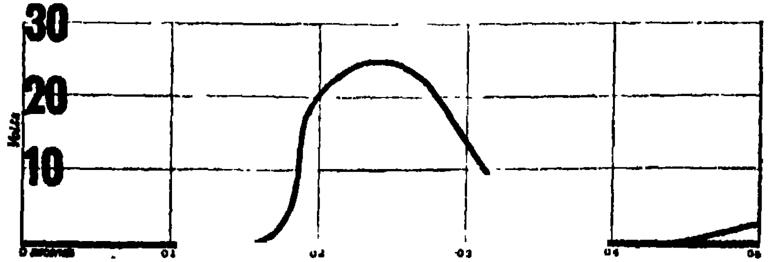
[*] Garten, 'Centralbl. f. Physiol.,' vol. 13, No. 1, p. 1.

[†] 'Phil. Trans.,' B, vol. 187 (1896), p. 381.

Since the constants of this particular electrometer had been accurately determined, it might have appeared feasible, after measuring the resistance of the preparation and the resistance of the shunt, to calculate the E.M.F. of the *Malapterurus* shock directly from the photographic record. Such a method would not, however, have been satisfactory, owing to the character of the experimental conditions obtaining in the present research, and it was deemed preferable to obtain the comparison curves given by an E.M.F. nearly comparable with that of the organ response, for the following reasons. The meniscus of the electrometer indicates the E.M.F. acting upon it at any moment, partly by its position above or below zero, and partly by the velocity with which it is moving at that instant, the position being measured in divisions of an arbitrary scale, and the velocity by the subnormal to the curve, expressed in polar co-ordinates. But the ratio between the value of a division on the subnormal and a division on the scale varies with the resistance in circuit, and is consequently affected by the use of a shunt. Obviously, therefore, the simpler method, and the one least open to objection, was to photograph the normal excursion given by an E.M.F. not many times smaller than that of the organ response through a circuit as nearly as possible similar to the actual one, *i.e.*, through an equivalent resistance and through the identical shunt The organ itself could not be employed for this purpose, because even if it gave no response to such a stimulus, the possible effects of electrolytic polarisation might impart to the record time-relations different from those of a normal excursion

We therefore carefully measured the resistance of the preparation with the leads in the position employed in obtaining the curves referred to previously (No. 3012 and 3013); we then substituted for it an equivalent non-polarisable, non-inductive resistance, and photographed the excursions given on throwing in a constant E.M.F. of 9 volts through this circuit with a shunt of three-lamps for comparison with the

FIG. 2.



Analysis of No. 3012, showing the E.M.F. of the single shock of 15 mm. of electrical organ, and representing about one-eighth part of the E.M.F. of the shock from the entire fish.

organ shock curve, No. 3012, and a shunt of two-lamps for comparison with the other curve, No. 3013. The curves were then analysed by the method described by one of us.

Both curves gave the same ratio, namely, 15 cm. on the subnormal = 88 scale-divisions on the radius vector. With the three-lamp shunt 120 scale-divisions = 9 volts.

These data, applied to the analysis of the organ shock curves, gave the results detailed in the annexed table. It will be noted that No. 3013, though a much weaker shock, is similar in its time-relations to No. 3012.

Electromotive Force of Shock obtained from 15 mm. of *Malapterurus electricus* Organ

Time after stimulus, in fractions of a second	Electromotive force, in volts (No 3012).	Electromotive force, in volts (No. 3013).
0·0150	0·00	0·00
0·0160	0·14	0·00
0·0170	1·68	0·00
0·0175	3·00	0·00
0·0180	5·20	0·59
0·0185	11·00	
0·0190	17·70	4·25
0·0195	19·40	
0·0200	20·50	8·60
0·0210	22·70	12·80
0·0220	24·00	14·90
0·0230	25·00	15·70
0·0240	25·10	16·15
0·0250	24·90	16·60
0·0260	23·80	16·80
0·0270	22·70	
0·0280	20·20	16·25
0·0290	17·70	
0·0300	14·20	13·85
0·0310	11·70	
0·0320	8·20	11·00
0·0330	6·00	
0·0340	4·90	7·85
0·0350	3·60	
0·0360	2·60	5·15
0·0370	1·75	
0·0380	0·79	3·59
0·0390	0·30	
0·0400	0·00	2·12
0·0440	0·00	
0·0460	0·66	0·00
0·0480	1·92	

In fig. 2 the results of one of the analyses (3012) given in the table are plotted as a curve. This curve may be compared with that of fig. 1, of which it is the interpretation. The ordinates represent the potential difference between two points, 15 mm. apart, in volts; the abscissæ represent time-intervals after the moment when the nerve was excited.

Comparing these results with those quoted on p 384 of our previous paper, it will be seen that the initial delay (0 0170 sec. and 0·0180 sec.) is longer than that obtained in our earlier experiments (0 010 sec.), and also that the duration of the organ response is longer (0·0390 sec. instead of 0·021 sec), although the temperature was nominally the same, viz, 5° C, in both investigations It must, however, be noted that in the previous research the organ was simply laid upon a glass stage kept at 5° C. by water flowing beneath it, whereas in the present case the preparation was completely enclosed in a large chamber cooled to 5° C.* Undoubtedly the actual temperature of the whole organ strip was higher than 5° C. in our earlier work, and we have ample evidence that in the case of nerve the time-relations of the electrical response are considerably affected by slight changes of temperature at or about 5° C. It should be further noted that the curve now obtained is on a much larger scale than any of those referred to in our previous research, and in consequence the small beginnings of the rise of E M F. can be detected at an earlier stage The curve is that due to the first, or initial, response of a series produced by self-excitation, the commencement of the second member of the rhythmical series caused by such self-excitation is indicated in the plotted curve (fig. 2), but since the object of the experiment was to determine the development of the initial shock, no attention was paid to the other members composing the organ discharge.

The following points brought out by the analysis must be dealt with in more detail.—

(1) There is no trace of any second phase of opposite sign. This characteristic of the organ response is in accord with all previous experiments. It is obviously associated with the circumstance that, since each disc with its nerve endings forms an independent system, no structural basis is furnished for the propagation of the excitatory change from one such system to its neighbours.

(2) The potential difference between the terminals rises more rapidly than it falls (rise, 0·0070 sec.; fall, 0·0160 sec). Since propagation in the organ does not exist, the rate of development and of decline in the potential difference is the nearest approach yet obtained to the time relations of a localised electrical response in an excitable tissue. It is magnified by the circumstance that such local response occurs almost simultaneously in a whole series of nerve endings. We regard the whole analysis as probably typical of the explosive electrical effect which is evoked in nerve endings when these are at 5° C., and are

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* For description of chamber, see Gotch and Burch, 'Journ. of Physiol.,' vol. 24, 1899.

excited by a single stimulus. The elimination of all propagation owing to the structure of the tissue, is a factor of great importance in this connection since such complete elimination is, in our opinion, not experimentally possible in the case of either muscle or nerve. Both the quicker rise and the slower fall may therefore be regarded as expressions of the character of the local change in the nerve endings.

Such a difference between the rate of development and of subsidence of the excitatory explosion was indicated in our earlier experiments, although not referred to in our published paper.* In those experiments we find, on examining carefully twelve different records, the following relation between the duration of the two states, development and subsidence.—

$$\frac{\text{Duration of development}}{\text{Duration of subsidence}} = \frac{265}{326}, \text{ or } \frac{81}{100}.$$

In the present instance, possibly owing to the more effectual cooling, the more prolonged character of the subsidence is very conspicuous.

Thus in the two analysed instances, here referred to, we find

$$\frac{\text{Duration of development}}{\text{Duration of subsidence}} = \frac{7}{16}, \text{ or } \frac{44}{100}, \text{ and } \frac{8}{17}, \text{ or } \frac{47}{100}.$$

One other point of interest in connection with the development of the E.M.F. is the comparatively gradual character of its actual commencement. The analysis shows that for 0·002 second after a potential difference can be detected, its value is relatively small.

It might be objected that the gradual development and still more gradual subsidence of the E.M.F. may have some purely physical explanation apart from the physiological change in the nerve endings, such, for instance, as polarization capacity due to the special structure of the tissue. That this is not the case is clearly proved by experiments in which a strong induction shock traversed the organ, which failed to excite it but was itself recorded on the plate. We have several examples of induction shocks of different intensities, of condenser discharges, and of excursions due to transient currents through the same circuit. In none of these is there any resemblance to the peculiar form of the curve given by the organ response. We are therefore in a position to say that the time relations of the organ shock do not resemble those of either induction currents, condenser discharges, or currents of short duration from a source of constant E.M.F.

(3) The duration of the period between the excitation of the nerve and the commencement of the organ response (0·017 sec.) represents the transmission time of the excitatory state along 40 mm. of nerve fibre when cooled to 5° C., this being the distance between the seat of

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* 'Phil. Trans.,' B, vol. 187 (1896), p. 347.

excitation and the organ strip. It is probable that the greater part of this time is occupied by the slow transmission of the excitatory state along the finest sub-divisions of the nerve within the organ near the ultimate nerve endings, all of which were at 5° C.

(4) The most interesting point in connection with the whole experiment is the maximum E.M.F. attained by the response given by the small portion of organ (15 mm.) investigated; this amounted to 25·10 volts in the most favourable instance.

The contacts were 15 mm. apart, and we convinced ourselves that localised excitatory changes in the piece of tissue situated between these contacts were responsible for the development of the electrometer movements when our apparatus was arranged as indicated in the opening description. This piece of tissue was subsequently removed and appropriately fixed for microscopic examination. Sections were then cut so as to display all the discs lying between the points of the electrode contacts. The recent work of Ballowitz has shown that the nerves do not reach the expanded discs, but end in their caudal stalks. The discs themselves are contained in the lozenge-shaped compartments constituting the columns. These are so situated that one columnar row of compartments is dove-tailed into those of all its neighbours. The result is that the number of discs and stalks in longitudinal series is twice as many as the number of lozenge-shaped compartments constituting any given column. Enumeration of the successive compartments in a number of different columns throughout the portion of organ between the contacts (15 mm. long) gave the following figures: 260, 265, 262, &c. It was therefore assumed that the electromotive difference of 25·10 volts was probably distributed uniformly over a series of at least 530 discs; the maximum E.M.F. of the change in any one disc, with its nerve endings, would thus be not more than 0·048 volt. It is of intesest to note that in the sciatic nerve of the frog we have obtained an excitatory effect amounting to 0·033 volt.

(5) The whole organ of the fish measured 12 to 15 cm. The extreme ends are thinned down, but it may be certainly inferred that 12 cm. of this organ would be at least as functionally active as the portion we investigated. This would give a development of 200 volts for the whole series of organ discs, and even this high value cannot be regarded as a maximum for the living fish, since it was evident to us that the organ preparation we employed had its functional activity lowered both by the low temperature and by the operations involved in its dissection. It is worth noting that during the first stages of the dissection carried out on the entire fish cooled in ice-cold water to anæsthesia, the division of a nerve branch with metal scissors whilst the organ was grasped by metal forceps, caused a strong shock to pass through the arms of the operator (F. G.), which was felt up to the elbows.

It must be self-evident to anyone who makes the experiment that the phenomena resulting from the passage of the organ shock through the human body are such as cannot be produced by interrupted battery currents unless the potential is high It is a matter of common knowledge that the shock from a healthy fish will pass through a chain of several people holding hands, and will be felt by each, not only in the arms, but in the muscles of the chest and shoulders. No battery current of 30 or 40 volts through such a circuit will do this, however interrupted, unless the circuit has considerable self-induction, in which case the E M F. of the battery does not represent the E.M F. of the shock, which may greatly exceed it.

It is, therefore, surprising that D'Arsonval's investigations led him to give 17 volts, and that Schonlein gives 31 volts as the maximum E.M F. of a *Torpedo* shock. Such low numbers indicate, we think, that the methods used by these observers were not applicable to the measurement of the maximum E M.F. of the shock of an electrical organ.* This opinion was stated definitely in our paper, and we here repeat the statement because the photographs now published show plainly that the development of the E M.F. of an organ shock is, at low temperatures, comparatively slow, much less rapid in fact than that of a battery current thrown in by breaking a short circuit. So far, therefore, as suddenness of change is concerned, the electrical organ is, under these conditions, at a disadvantage as compared with a battery current; yet it can produce muscular contractions such as can only be caused by interrupted currents of high potential. We are, therefore, constrained to believe that the maximum E.M.F., even in *Torpedo*, will be found to be nearer 200 than 30 volts. At any rate the analysis of the foregoing curves indicates that this maximum, namely, 200 volts, is attained by the organ shock of *Malapterurus electricus*.

The Electrical Resistance of the Electrical Organ.

Owing to the failure of the organ to respond to further excitation, it was impossible to carry out the other experiments which we had contemplated. We therefore decided to make such measurements of the electrical resistance of the tissue as could be effected with the apparatus at hand. In the absence of a Kohlrausch bridge we employed a resistance-box of the Post Office type, and used the capillary electrometer as an indicator. With this instrument it is advantageous to have the bridge arms of the highest available resistance—in this case 1000 ohms : 100 ohms—as the current is thereby reduced, while the excursions are in no way lessened. For a similar reason a shunt is not employed, but the current is derived from a rheocord instead of a battery, the potential being kept low until a balance is nearly obtained. Square blocks of the organ of various dimensions were placed on a glass slip, and broad cables of lamp-wick pasted over with kaolin and salt solution used to connect them with the ordinary non-polarisable electrodes. After each measurement the cables were joined together without the interposition of the organ strip, in order to ascertain the resistance due to the leads. It was found that the variation in resistance of the leads between one experiment and another was relatively inconsiderable, the greater part of the electrode resistance being evidently due to the unexposed portions of the non-polarisable electrodes, *i.e.*, the tubes containing saturated zinc sulphate. The direction of the current was reversed from time to time, but such reversal was not found to exercise any marked influence upon the results

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* D'Arsonval, 'Comptes Rendus,' vol. 121, p. 145, 1895; Schönlein, 'Zeitsch. f. Biol.,' vol. 31, N. F., 13.

The differences in the extensibility and elasticity of the superficial and deep boundary walls of the organ offered a difficulty, since it was found to be almost impossible to cut the organ into blocks which should be of the same superficial area on these two aspects. Care was, however, taken that in every case the dimension in the direction of the length of the columns (*i.e.*, head end to tail end) should be, if anything, less than that in the direction across the length of the columns (*i.e.*, transverse). These two dimensions will for brevity be termed, the first, longitudinal, the second, transverse, the words indicating their relationship to the organ column.

It must be remembered that the line of flow of a current directed longitudinally is transverse to all the flattened discs which are placed athwart the columns—whilst that of a current directed transversely to the column is parallel with these thin discs. On *primâ facie* grounds we should expect that the resistance in the former case would be far larger than that in the second if, as seemed certain, the thin disc substance has an electrical resistance which is far above that offered by the remaining space of the compartment and the albuminous substance with which this is filled.

This expectation was fully realised by the experimental results, of which the following table gives examples —

Resistance of Block of Electrical Organ.

Dimensions.			Resistance.	
Longitudinal.	Transverse.	Thickness.	To longitudinal current flow	To transverse current flow.
mm.	mm.	mm.	ohms.	ohms.
10	10	3	2700	600
15	15	3	2800	1300
12	13	3	3100	1300

The resistance to the flow of a current in the longitudinal direction (*i.e.*, directed across the disc surfaces) is thus from two to three times as great as that offered by the organ to the flow of a current in the transverse direction (*i.e.*, directed parallel to the disc surfaces). The discs themselves thus offer, when their physiological condition is unimpaired, a high resistance as compared with the adjoining compartmental contents, and this result is corroborated by experiments made after the conditions had been modified either physically or physiologically. The physical modification consisted in taking blocks, the longitudinal dimensions of which varied. Thus a block 10 mm. in the transverse dimensions and 3 mm. in thickness, was cut so as to be 90 mm. in the longitudinal dimension. Its resistance to the flow of a longitudinal current amounted to 27,600 ohms On reducing its length to 40 mm. the resistance was 13,000; on reducing it to 20 mm. it was 5700, and on reduction to 10 mm. it was 2700 ohms. The heavy longitudinal resistance is seen to increase in proportion to the length of the columns, and the results thus indicate that our method was a fairly accurate one

The physiological modifications were produced both by destroying the living condition of the fresh tissue by a suitable rise of temperature and by keeping the tissue in physiological saline for a number of hours, so that the living condition should be more or less replaced by one due to commencing natural death.

In the case of destruction through heat, the striking discrepancy between the large resistance offered to longitudinal currents, and the lower one offered to transverse ones, always disappeared completely. The resistance was now found to be the same whatever the direction of the current flow

In the case of kept preparations, the disparity between the two resistances became so much the less marked as the preparation lost its living characteristics; thus a preparation which had been kept 24 hours in saline still showed 1500 ohms longitudinal resistance, as compared with 500 ohms transverse, whilst a second strip, kept for 48 hours, showed only 1000 ohms longitudinal, as compared with 800 ohms transverse.

There is thus little doubt that the greater resistance offered by the columns of the fresh organ to longitudinal flow of currents is due to the circumstance that these are directed through the protoplasmic substance of the thin plates or discs, which, lying directly athwart the columns, are all so interposed in the line of flow as to give a maximum of protoplasm to be traversed by such a flow. On the other hand the small resistance offered to transversely directed currents is an expression of the fact that the flattened protoplasmic discs now form but an insignificant portion of the conducting medium, which is chiefly composed of the compartment spaces. The discs must therefore have a relatively high resistance as compared with that of the albuminous fluid filling the remainder of the space It has been sometimes suggested that alterations of resistance may play an important part in the phenomena of organ activity The experiments just given appear to indicate that the discs have a resistance which is of a different order to that of the physiological saline in the surrounding media, but even in the case of these protoplasmic structures the results scarcely warrant the belief that there is anything exceptional in their higher resistance since it only places them in the same category with such other excitable tissues as muscle and nerve, which have been shown to offer a greater electrical resistance in the transverse than in the longitudinal direction

"On the Formation of the Pelvic Plexus, with especial Reference to the Nervus Collector, in the Genus *Mustelus*" By R. C. PUNNETT, B.A., Scholar of Gonville and Caius College, Cambridge Communicated by HANS GADOW, F.R.S

Received June 30,—Read November 16, 1899

(Abstract)

The main object of this investigation was to ascertain whether at any period in the development of the animal selected, the number of branches composing the *nervus collector* was greater than that found in the adult. As a logical consequence of Gegenbaur's theory we should expect such to be the case, and the ontogenetic history of the *nervus collector* recorded in this paper, its maximum development in young embryos, and its subsequent gradual decrease through the later stages of embryonic existence leading to its condition in the adult, must, if there is any truth in the recapitulation theory, all point to its primitive character.

The history of the posterior collector, the very existence of which has not hitherto been described, throws important light upon the theory mentioned above. Here we have a collector formed in the embryo, from which in later stages the component nerves separate and run singly into the fin. Such a fact points very strongly to the collector condition being more primitive than that condition in which the nerves reach it without previously effecting any junction with one another

It is further shown that the formation of this collector is due to migration of the whole fin rostrally, and not merely to a contraction of the fin area, and in support of this the following evidence is brought forward. The two species, *M. lævis* and *M. vulgaris*, differ from one another chiefly in the more rostral position of the pelvic girdle in the former. That it is highly improbable such a condition should be due

to excalation of vertebræ between the pelvis and head region of *M. lævis* is shown in such facts as the following:—

(*a*) The great amount of both excalation and intercalation which must be going on in different regions of the animal on such a hypothesis

(*b*) In some cases the girdle-piercing nerve may pass partly over and partly through the girdle, not showing that rigidity which on the excalation theory we should be led to expect.

(*c*) The serial number of the girdle-piercing nerve may be different on the two sides of the same individual

On the hypothesis of migration such facts receive an easy explanation, which is also in accordance with the existence of a greater caudal extension of the area of innervation of the pelvic fin in the males of *M. lævis* than the females, and in the great amount of variability in *M. lævis*, which species we suppose to have been derived from a more stable form such as *M. vulgaris* by a rostral migration of the pelvic girdle.

Hence migration being rendered very probable on other grounds, the posterior collector must be supposed to be formed as a direct result of that migration, and its undoubted connection with the shifting of the fin along the vertebral column is of great importance in explaining the formation of the anterior *nervus collector*.

"On the Least Potential Difference required to produce Discharge through various Gases." By the Hon. R. J. STRUTT, B.A., Scholar of Trinity College, Cambridge. Communicated by LORD RAYLEIGH, F.R.S. Received October 17,—Read November 16, 1899.

(Abstract.)

The investigation, of which an account is given in this paper, deals with the potential difference required to produce sparks in various gases, between large parallel planes at a fixed distance apart, and at various pressures.

It was found by Mr. Peace* that the striking potential between two parallel plates in air diminished as the pressure diminished, till a certain point was reached, and then began to rise very rapidly. The pressure at which the striking potential was a minimum, depended on the distance between the plates, and increased as the distance was lessened. The minimum potential itself, however, varied very little with the distance between the plates.

This minimum potential was of the same order as the cathode fall of potential in air, as has been pointed out by Professor J. J. Thomson.* The following explanation may be offered of the fact that there is this minimum striking potential, and that it is approximately constant.

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* 'Roy. Soc. Proc.,' vol. 52, p. 99.

The negative glow in any gas, as has been shown by Warburg,† requires for its production a definite difference of potential (about 340 volts in the case of air) independent of the pressure and constant, so long as the glow is not crushed into a smaller space than that which it would naturally occupy. If the glow is crushed, the necessary potential is greater

Let us now suppose that the discharge takes place between two parallel plates. A part of the space between these plates is occupied by the negative glow, a part by the positive column. So long as any of the positive column remains, it is clear that the negative glow is not constricted, and consequently it only requires 340 volts to produce it. The greater the length of the positive column, the greater the corresponding potential difference, so that the striking potential will be the least possible when the pressure is low enough to make the negative glow occupy the whole space between the plates, but not low enough to make it require more.

My experiments have been undertaken with a view to obtaining further experimental evidence on these ideas. The sparks were taken between large metal plates, ¼ mm apart. For details of the apparatus and method of experimenting, the original paper must be consulted. There also will be found curves showing the relation between spark potential and pressure for the following gases: atmospheric air, hydrogen, nitrogen, helium.

I give here only the minimum value of the spark potential found for each, together with the cathode fall of potential given by Warburg:—

Nature of gas.	Cathode fall	Minimum spark potential.
	volts.	volts.
Atmospheric air........	340—350	341
Hydrogen..............	300	302, 308
Ordinary nitrogen.......	Variable, 315—340	347, 351, 369, 388
Nitrogen specially freed from all traces of oxygen	230	251
Helium................	226	Variable, 320—261

It will be seen that on the whole, the evidence is in favour of the views explained above.

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* 'Recent Researches in Electricity and Magnetism,' p. 158.

† 'Wied. Ann,' vol. 31, p. 579.

It was found impracticable to get accurately consistent results in the case of helium. Some cause, the nature of which has not been traced, made the results differ with different samples of the gas, although in each case care had been taken with the purification. The helium curve, however, shows very peculiar features, the spark potential being, for a wide range of pressure in the neighbourhood of the minimum, almost independent of the pressure.

November 23, 1899

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

Professor Edward Divers (elected 1885) was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair, and the list of Officers and Council nominated for election was read as follows:—

President.—Lord Lister, F.R.C.S., D.C.L., LL.D.

Treasurer.—Alfred Bray Kempe, M.A.

Secretaries.—Sir Michael Foster, K.C.B., D.C.L., LL.D.
Professor Arthur William Rucker, M.A., D.Sc.

Foreign Secretary.—Thomas Edward Thorpe, Sc.D., LL.D.

Other Members of Council.—Horace T. Brown, F.C.S.; James Bryce, D.C.L.; Captain Ettrick William Creak, R.N.; Professor James Dewar, M.A., Professor Edwin Bailey Elliott, M.A.; Hans Friedrich Gadow, Ph.D.; Professor William Dobinson Halliburton, M.D.; Professor William Abbott Herdman, D.Sc., Sir Andrew Noble, K.C.B.; Professor Arnold William Reinold, M.A.; George Johnstone Stoney, D.Sc.; George James Symons, F.R.Met.Soc., J. J. H. Teall, M.A.; Professor Joseph John Thomson, M.A.; Professor Edward Burnett Tylor, D.C.L.; Sir Samuel Wilks, Bart., M.D.

The following telegram from Her Majesty's Astronomer at the Cape of Good Hope was read:—"Lines of Beta Crucis 4552, 4569, 4575, described unknown in my April paper, Lunt finds due to silicon. Paper follows."

The following Papers were read:—

I. "Note on the Spectrum of Silicium." By Sir J. NORMAN LOCKYER, K.C.B., F.R.S.

II. "Preliminary Table of Wave-lengths of Enhanced Lines." By Sir J. NORMAN LOCKYER, K.C.B., F.R.S.

III. "The Colour-Physiology of *Hippolyte varians.*" By F. W. KEEBLE, and F. W. GAMBLE. Communicated by Professor S. J. HICKSON, F.R.S.

IV. "The Medusæ of *Millepora.*" By Professor S. J. HICKSON, F.R.S.

"Note on the Spectrum of Silicium." By Sir NORMAN LOCKYER, K.C.B., F.R.S. Received November 9—Read November 23, 1899.

In 1895, during the course of an investigation of the spectra of gases distilled from the mineral Eliasite, a double line at λλ 4128·3 and 4131·4 was found in one of the photographs, which could not at the time be traced to any terrestrial substance. It was thought that it might belong to some new gas, especially as there was a well-marked double in the corresponding region of α Cygni.

Some time later, shortly after the discovery by Professor Pickering of a new series of probable hydrogen lines in the spectrum of ζ Puppis, an attempt was made to produce this series of lines in the laboratory, and the spectrum of hydrogen was examined under different electrical conditions. During this research it was found that with the use of the spark in vacuum tubes of very narrow bore, with large jars in circuit, the same double line noted in the eliasite photograph made its appearance, and as in these experiments the glass on the inside of the capillary of the vacuum tube had become partially fused, silicium suggested itself as being the origin of this so-called "unknown" double. That this was correct was proved directly afterwards by photographing the spectrum of a spark over SiO_2 in a retort, the double in question being the most prominent feature of the spectrum.*

In addition to this double, a wider one at λλ 3856·1 and 3862·7 was noticed, and as the two components also agreed very closely in position with lines in the spectrum of α Cygni, it was concluded that silicium was the true origin of the lines.†

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* A list of the spark lines of silicium was published by Eder and Valenta in 1893, and the identity of the strange double would probably have been established before by a reference to that list, had it not been for a large error in the wavelength of one of the components of the double as recorded by them.

† The probable explanation of the appearance of the silicium double in one of the photographs of the spectrum of the eliasite gases is that one of the platinum

Very few records of work on the silicium spectrum have been published by later spectroscopists, but Eder and Valenta* give lines agreeing in wave-length with those mentioned, as shown in the accompanying table.

λ. (Lockyer).	Int.	λ. (Eder and Valenta).	Int.	Remarks.
3856·1	6	3855·7	3	
3862·7	4	3862·5	3	
4128·1	3	4126·5	4	Probably a misprint for 4128·5
4131·1	4	4131·5	4	

Later experiments on the spark spectrum of silicium with the aid of the large Spottiswoode coil, and on the spectra of silicium compounds in vacuum tubes, reveal other lines of that element no less interesting from a stellar point of view than those previously mentioned.

We learn from these recent observations that the lines of silicium may be divided into three sets, no two of which behave alike under varying electrical conditions. The wave-lengths of the lines composing the different sets are:—

$$\left.\begin{array}{l}3856{\cdot}1\\3862{\cdot}7\\4128{\cdot}1\\4131{\cdot}1.\end{array}\right\}\text{A.}\qquad\left.\begin{array}{l}4089{\cdot}1\\4116{\cdot}4\end{array}\right\}\text{B.}\qquad\left.\begin{array}{l}4552{\cdot}8\\4568{\cdot}0\\4575{\cdot}3\end{array}\right\}\text{C.}$$

There is a line at λ 3905·8 which is associated in the spark spectrum of silicium with the lines in set A, but while these are entirely absent from the arc spectrum of silicium, 3905·8 is a strong line in the arc spectrum. This line differs from the others, therefore, in not being enhanced in intensity in passing from the conditions of the arc to those of the spark. So far as is known, the lines in sets B and C have not been recorded by any other observers of the silicium spectrum.

The behaviour of the three sets of lines in terrestrial spectra is shown in the following figure.

It is found, on investigating the occurrence of these silicium lines in stellar spectra, that the three sets of lines respectively attain a maximum intensity at the three different levels of stellar temperature represented by β, γ, and ζ Orionis.

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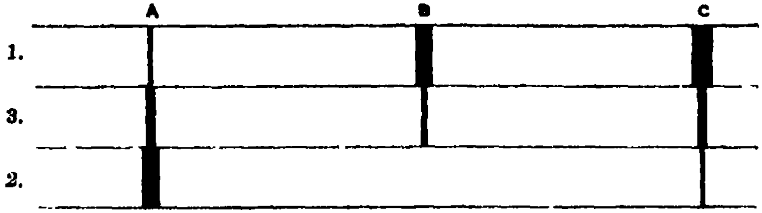
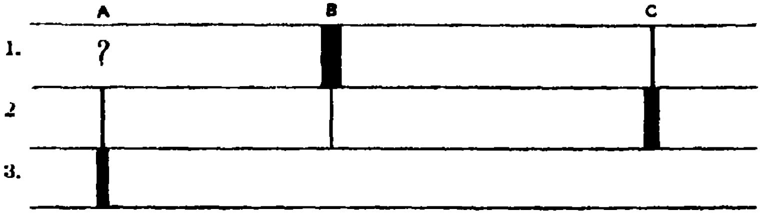
poles of the "steeple" used in that particular case had broken off close to the glass, and the latter was fused by the heat from the spark. This is the more likely as the double line only appeared on one edge of the spectrum.

* 'Denkschr. Kais. Akad. Wissensch., Wien.,' vol. 60, 1893.



1. SiF_4 vacuum tube (capillary).
2. SiO_2 spark with Spottiswoode coil.
3. Vacuum tube (bulb) SiO_2 spark with small coil.

The accompanying figure shows the behaviour of the different sets A, B, and C in the spectra of β, γ, ζ Orionis.



1. ζ Orionis.
2. γ Orionis.
3 β Orionis.

We find that set A is most prominent in the spectrum of β Orionis, that set C predominates in the spectrum of γ Orionis, and that set B is by far the strongest in that of ζ Orionis.

That the stars named represent three different grades of temperature, ζ Orionis being the hottest, and β Orionis the coolest, has been previously deduced by the discussion of other lines in their spectra. This result was embodied in a paper "On the Order of Appearance of Chemical Substances at different Stellar Temperatures," which I read to the Society in February of the present year. In that paper α Crucis was given as a typical star representing a stage of temperature between those of β Orionis and ζ Orionis. That star can be very well replaced for the purpose of the present discussion by γ Orionis, the two spectra being nearly identical.

The line at λ 3905·8 previously mentioned as occurring in both arc and spark spectra, is not represented in the spectra of any of these stars. It is possibly present in the spectra of stars like the sun, as Rowland records it in his "Preliminary Table of Solar Spectrum Wave-lengths," as being coincident with the well-marked Fraunhofer line at λ 3905·660. This coincidence, however, is open to doubt; from a comparison of the Rowland grating photographs of the silicium spark spectrum and the solar spectrum taken at Kensington, the silicium line apparently agrees better in position with the less refrangible edge of the solar line than with the middle.

Before this point can be definitely settled, still larger dispersion will have to be employed.

In the paper mentioned, it was shown that silicium made its appearance first at the temperature represented by α Ursæ Minoris, and strengthened at the higher temperature of α Cygni and β Orionis, afterwards weakening as we pass through the still higher temperatures of ζ Tauri and γ Orionis, until at the ζ Orionis stage it is bordering on extinction

In the same paper the behaviour of a line at λ 4089·2 was plotted, and at the same time it was quoted as an "unknown" line.

It is interesting to note that this line is now traced to silicium, and is the strongest line in set B. It is apparently a short-lived line in stellar spectra, as it only occurs between the stages of temperature represented by γ Orionis and ζ Orionis, being one of the weakest lines in the spectrum of the former star, and one of the strongest in that of the latter.

Most of the photographs of the silicium spectrum under varying conditions were taken by Mr. Butler. The wave-lengths of the lines have been reduced by Mr. Baxandall, and he is also responsible for establishing the identity of the terrestrial and the stellar lines. My thanks are due to him also for help in the preparation of the present communication.

Preliminary Table of Wave-lengths of Enhanced Lines." By Sir Norman Lockyer, K.C.B., F.R.S. Received November 9,—Read November 23, 1899.

Introduction.

In the year 1881 I communicated a paper* to the Royal Society in which I described some experiments relating to the brightening of some lines in the spectrum of iron on passing from the arc to the spark.

It was found that in the case of iron, the two lines in the visible spectrum at λ 4924·1 and λ 5018·6, on Rowland's scale, were greatly enhanced in brightness, and were very important in solar phenomena.

The work was subsequently carried into the photographic region of the spectrum with very interesting results, since it was found that several other lines were enhanced at the highest temperature I could then obtain.

In a later paper† I described the results obtained in further photographic investigations of metals at high temperatures, dealing specially with the spectra of iron, calcium, and magnesium, and more recently still,* I referred to the enhanced lines of other substances, but refrained from giving a list of the wave-lengths of the lines photographed, as the series of comparisons with the large Rowland grating was not then completed.

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* 'Roy. Soc. Proc.,' 1881, vol. 32, p. 204.

† 'Roy. Soc. Proc.,' vol. 61, p. 158.

The important part which the enhanced lines of the elements play in the study of stellar spectra cannot be over-estimated, but a great advance can only be made in this direction by a systematic examination of the spectra of all the elements. Such an undertaking as this involves considerable time and labour. I have been fortunate enough to have the use of the large 42-inch Spottiswoode coil for a short space of time, and employed it in this work, for which it is specially adapted, as the brilliancy of its spark shortens the time of exposure. Although I have previously stated my indebtedness to Mr. Hugh Spottiswoode and Mr. G. Matthey for their assistance, I wish again to express my best thanks to them, and I must now add Professor Moissan and Sir William Crookes, who have kindly supplied me with some specimens of metals.

The elements which have been dealt with in this investigation are the following :—" Aluminium, bismuth, chromium, copper, iron, magnesium, manganese, titanium, and vanadium."

For each of these elements the spark and arc spectra were photographed and compared, and the wave-lengths of the enhanced lines, that is, those lines which are intensified in passing from the temperature of the electric arc to that of the spark, were determined.

Method of Reduction.

The method of reduction was as follows :—The spark spectrum of the element was first compared directly with the spark spectrum of air between platinum poles, and the air lines were thus eliminated. The spark and arc spectra of the element being taken on different plates, were then compared, and the lines present in the spark spectrum but absent from the arc, or lines relatively more intense in the spark than in the arc spectrum, were noted. The wave-lengths of these lines were then determined by direct comparison with a solar spectrum taken under the same instrumental conditions, and reference to Rowland's list of lines in the normal solar spectrum.

Instruments Employed.

The grating used is a 6-inch concave one, having a surface 2 inches by 5¾ inches, ruled with 14,438 lines to the inch, and a radius of curvature of 21 feet 8½ inches. The instrument is mounted in the

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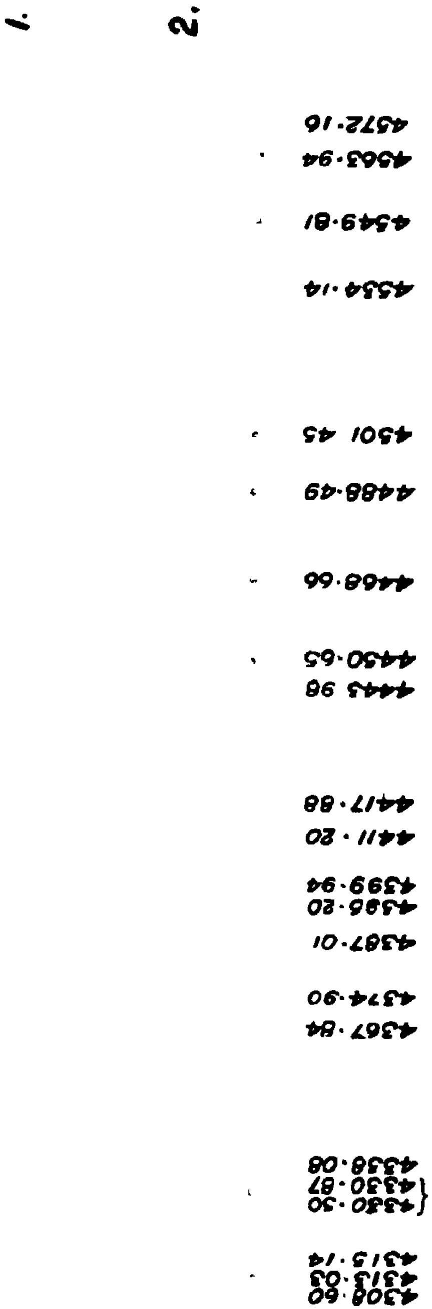
* 'Roy. Soc. Proc.,' vol. 61, p. 441.



Titanium.—Enhanced Lines.
1. Arc 2. Spark.

manner described by Rowland, the camera and grating being at opposite ends of an iron girder adjusted exactly to the radius of curvature of the grating. It has been found that by carefully making the adjustment the scale can be maintained constant within a very small limit, and the conditions were such that it was possible to use plates 18 inches long without bending

In some cases a Steinheil prism spectroscope was used. The dispersion arrangement of this instrument consists of four dense flint prisms, three of 45° angle and one of 60°. These are fed by a collimator of $1\frac{1}{2}$ inches aperture and $18\frac{1}{2}$ inches focal length. The camera objective is a single quartz lens of 2 inches aperture and about 54 inches focal length (for λ 4340), the non-achromatism of which necessitates a considerable inclination of the photographic plate to the axis of the lens. The total deviation for the blue region of the spectrum is about 150°. The scale of the spectrum is as follows :—

$$\text{D}-\text{F} = 2\tfrac{7}{8} \text{ inches.}$$

$$\text{F}-\text{K} = 6\tfrac{7}{8} \quad ,,$$

The spark conditions were as follows :—The Spottiswoode coil is capable of giving a spark 42 inches long in air. For spectroscopic purposes, however, a condenser is inserted in parallel with the secondary circuit, the length of spark then obtainable depending on the capacity of the particular condenser used. These have varied from a single gallon jar to a battery of twelve jars of about 15 gallons each, and finally a plate condenser has been used, at the suggestion of Professor Boys.

This consists of thirty sheets of plate glass, 30 inches by 25 inches, with tinfoil, 24 inches by 12 inches, between each pair. The spark under these conditions varies from about 25 to 2 mm. in length, and this was again further controlled in intensity and duration by a secondary spark gap in series with the one containing the metallic poles.

The primary was fed from the street circuit at 100 volts, the usual current employed being about 25 amperes. Interruption of the current in the primary was by means of a mercury break actuated by hand.

In the case of the arc, the exposures lasted generally for about fifteen minutes, while an hour and a quarter was the average time given for the spark.

My thanks are due to Mr. C. P. Butler, who was employed in taking the photographs, to Mr. F. E. Baxandall, who is responsible for their discussion, and to Dr. Lockyer for assistance in the preparation of this note. The enlargement of portions of the arc and spark spectra of titanium was made by Sapper J. P. Wilkie, R.E.

The Tables.

In the following tables, in which the elements are arranged alphabetically, will be found the wave-lengths of all those lines which have been observed as enhanced in the region examined.

The first column gives the wave-lengths of the enhanced lines, the second and third their intensities in the spark and arc respectively (maximum intensity = 10), and the fourth column is devoted to remarks.

In the case of iron and copper I give in addition the wave-lengths of the spark lines obtained by Herren Exner and Haschek* and Eder and Valenta respectively.†

Aluminium.

λ.	Int. in spark	Int in arc.	Remarks.
3900·68	5	0	Enhanced line of Ti at λ 3900 68.
4480 00	6	0	
4513·20	9	0	
4529 80	10	0	Enhanced line of Ti at λ 4529 6.
4663·70	10	0	

Bismuth.‡

λ.	Int. in spark.	Int. in arc.	Remarks.
3846·1	3	0	
3849·3	5	0	
3864 5	8	0	
4079·3	8	0	
4081·6	2	0	
4245·3	3	0	
4259·8	10	0	
4272·6	3	0	
4302·4	9	0	Enhanced Ti line at λ 4302·09
4308·8	2—3	0	" " λ 4308·60
4328·6	5	0	
4340·7	5	0	
4387·0	3	0	Enhanced Ti line at λ 4387·01.
4391·6	3	0	

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* W. Marshall Watts, 'Index of Spectra,' Appendix J, p. 2.

† *Ibid.*, Appendix H, p. 38.

‡ The wave-lengths are only given to five figures, as greater accuracy cannot be obtained owing to the great breadth and fuzziness of the lines.

Chromium.

λ.	Int in spark	Int. in arc	Remarks.
4088·20		0	
4242·62		trace	
4262·14		0	
4284·38		0	
4558·83	4	2	
4588·38	4	1	
4592·50	trace	0	
4619·71	1—2	trace	
4634·25	?	1	Intensity in spark difficult to determine owing to superposition of an air line.

Copper.

λ. Lockyer	Int. in spark	Int. in arc.	λ. Eder and Valenta	Int in spark.	Remarks.
4545·1*	2	0			
4556·10	5	0	4555·94	1*n*	Enhanced line of Fe at λ 4556·10.

* Seen only in spectrum taken with 4-prism Steinheil spectroscope.

Iron.

λ Lockyer.	Int. in spark.	Int in arc	λ. Exner and Haschek	Int. in spark	Remarks.
3839·78	2—3	0	3839·87	2	
3846·55	2—3	1	3846·54	2	
3868·87	3	1—2	3863·86	1	
3871·86	4	1—2	3871·88	3	
3906·04	1	0	3906·2	1*n*	Si spark line at λ 3905·70.
3935·92	5	4	3935·90	2	
3939·28	1—2	0	3939·06	1*n*	
4002·77	1—2	trace	4002·75	1	
4048·82	3—4	2	4049·03	1*n*	
4055·63	3	2	4055 58	1	
4173·52	3	1—2	4173·59	2	Enhanced line of Ti at λ 4173·70.
4178·95	3—4	trace	4179 01	2	
4233 25	4—5	0	4233·26	4	
4296·65	2	0	4296 73	1	
4302·35	2—3	2	4302·32	1	Enhanced line of Ti at λ 4302·09.
4351 93	5	0	4351 89	2	
4385 55	3—4	trace	4385·55	1	
4451·75	3	2	4451·70	1*n*	
4462·30	2	0	4462·15	1*n*	
4489 35	1	0	4489·34	1*n*	
4491·57	2	0	4491·58	1	
4508·46	5	trace	4508·42	2	
4515·51	4	”	4515 49	1	
4520·40	3	1	4520·41	1	
4522·69	6	2	4522·80	2	
4541·40	3	1	4541 68	1	
4549·64	7	1	4549·65	3	Enhanced line of Ti at λ 4549 81.
4556·10	5	0	4556 04	1	„ „ Cu at λ 4556·10.
4576·51	1	0	4576·48	1	
4584·02	8	1	4584·01	4	
4629·60	4	0	4629·51	1	
4635·40	3	0	4035 50	1*n*	
*4924·11	8	0	..	..	Exner and Haschek's observations do not extend to this region.
*5018·63	7	1	..	..	
*5169·07	6	2	..	..	
*5169·22					
*5316·79	3	0	..	..	

* Reduced from photograph taken with two 6-inch objective prisms.

Magnesium.[*]

λ.	Int. in spark.	Int. in arc.	Remarks
4395·0	1	0	Enhanced line of Ti at λ 4395·20.
4481·3	10	0	

Manganese.

λ.	Int. in spark.	Int. in arc.	Remarks
4000·20	2	0	
4105·05	3	0	
4128·36	2—3	trace	Strong Si spark line at λ 4128·1.
4137·16	3—4	0	
4200·40	2	0	
4206·56	4	0	
4342·45	4	0	
4244·43	1—2	0	
4248·10	1	0	
4251·86	5	0	
4253·13	5	0	
4259·35	4	0	
4292·35	2—3	0	
4300·37	2	0	Enhanced line of Ti at λ 4300·21.
4326·82	5	0	
4344·19	8	0	Enhanced line of Ti at λ 4344·15
4348·62	2	0	
4365·50	1—2	0	
4478·86	2—3	0	

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[*] The wave-lengths are only given to five figures, as greater accuracy cannot be obtained owing to the broad and fluffy nature of the lines.

Titanium.

λ.	Int. in spark.	Int. in arc.	Remarks
3900·68	10	4	
3918·61	10	4	
3932·16	4	trace	
3987·75	1	0	
4012·54	5	1	
4025·29	3	1	
4028·50	6	1	
4053 98	5	trace	Enhanced line of V at λ 4053·80.
4055·19	2	1	
4161·70	2—3	0	
4163·82	10	2	
4172·07	10	1	
4173 70	3	0	Enhanced line of Fe at λ 4173·52
4174 20	2	0	
4184·40	1	0	
4227·40	2	0	
4290·38	6	2	
4294 20	7	3	
4300·21	6	1—2	
4302·09	3	1—2	Enhanced line of Fe at λ 4302·35.
4308·60	7	1—2	
4313·03	7	1—2	
4315·14	8	1	
4316·96	2	0	
4321·20	3	1	
4330·50	2	trace	
4330·87	2	„	
4338·08	8	4	
4341·53	3	1	
4344·45	3	1	Enhanced line of Mn at λ 4344·19.
4351·00	2	0	
4367·84	5	1	
4374 90	3	0	
4387·01	5	trace	
4391·19	1—2	„	
4395·20	9	5	Enhanced line of Mg at λ 4395·0.
4396·01	2	trace	
4399·94	7	3	
4411·20	5	trace	
4417·88	6	2	
4421·93	3	2	
4443·98	0	4	
4450·65	3	1	
4464·62	3	1	
4468 66	9	4	
4488·49	5	1	
4501·45	8	4	
4529·60	3	trace	
4534·14	5	2	
4549·81	8	4	Enhanced line of Fe at λ 4549·64.
4563·94	7	3	
4572·16	9	4	
4590·13	3	1—2	

Vanadium.

λ.	Int. in spark.	Int. in arc.	Remarks.
3827·30	2–3	trace	
3867·00	3	2	
3878·90	8	4	
3885·05	3	2	
3899·30	6	3	
3903·40	6	4	
3914·44	6	4	
3916·50	6	4	
3952·07	7	5	
3973·85	5	4	
3985·90	1	0	
3997·30	5	4	
3909·30	2	0	
4005·85	10	7	
4017·00 4017·40	1	0	
4023·60	9	7	
4035·80	8	6	
4053·80	2–3	trace	Enhanced Ti line at λ 4053·98.
4061·80	2–3	1	
4065·20	3–4	0	
4178·50	1–2	1	
4183·60	4	3	
4202·55	3	2–3	
4205·24	4	3	
4225·41	3	1–2	
4232·20	1–2	0	
4248·10	1	0	

"The Colour-Physiology of *Hippolyte varians*." By F. W. KEEBLE, Caius College, Cambridge, and F. W. GAMBLE, Owens College, Manchester. Communicated by Professor S. J. HICKSON, F.R.S. Received October 25,—Read November 23, 1899.

The following paper gives in a categorical fashion the chief results of a research on the changes of colour in the prawn *Hippolyte varians*. The work was carried out last year partly in the Zoological Laboratories of Owens College, Manchester, partly at the station furnished by the Lancashire Sea Fisheries Committee at Barrow; and during the past summer in M. Perrier's Laboratory at St. Vaast, Normandy. A fuller description of the experiments, together with figures, will appear shortly. The present abstract contains the following sections:—

I. Previous knowledge of colour-change in *Hippolyte varians*.
II. Methods adopted for obtaining reliable colour records—
  a. Colour registration.
  b. Chromatophore examination.

III. The nature of the "chromatophores" and their pigments.
IV. The habits of *Hippolyte varians*. Sexual dimorphism.
V. The nocturnal colour. Nocturnes.
VI. Periodicity of colour-change.
VII. Range of colour-change.
VIII. The causes of change in colour.
  a. Colour of the surroundings.
  b. Light-intensity.
  c. Electric and other stimuli.
IX. The rôle of the eye and nervous system in the control of the colour-form.
X. The "chromatophores" of larval forms

I. *Previous Knowledge.*

The facts previously known may be arranged in three groups. The great variety of colour displayed by different specimens of *Hippolyte varians*; the "mimetic resemblances" between these colour-forms and the Algæ upon which they live, and the power which these colour-forms possess of undergoing a change of tint under different conditions of illumination. It is known that the so-called "chromatophores" contain differently coloured pigments by which both the prevalent tint of the prawn and its colour-changes are determined. The arrangement of these colour-elements in the body and the pigmentary conditions of the various colour-varieties have not been hitherto carefully examined. The factors which determine a change of colour, the extent of these changes, and the mode in which they are effected, may be said to be hitherto quite unknown.

II. *Method of Obtaining Reliable Colour-records.*

(*a*) Records of colour must be made under constant conditions of illumination, otherwise they are not strictly comparable, and fine shades of colour escape notice. They must be made rapidly, otherwise the light used for recording induces a colour-change and becomes, instead of a guide, a source of error. Finally, the light must be such as to enable a speedy record to be made, and yet one which itself induces a minimum change.

Many devices have been tried, but no completely satisfactory method has been obtained. We use, as most convenient, bright diffuse sunlight reflected from a white ground; and for a comparison of day and night colours, incandescent light. There are, however, several objections to these modes of illumination, the chief among them being that under certain circumstances, white light produces a very rapid change of colour.

(*b*) During microscopic examination, grave colour-changes often occur; yet with practice, a very rapid examination may be made, and so the source of error considerably reduced. The colours to be recorded are often several, the gross colour of the animal being seen under the microscope to be due to several pigments (see below Sect. III). These pigments are differently distributed in different colour-forms, so that the pigmentary records become rather complicated. Any but the briefest microscopical examination throws the nervous system of the animal out of gear, produces after-effects, and too frequently renders the animal useless for further experiment. Control-specimens must be used before conclusions can be drawn from the simplest experiments, and experiments must be confirmed several times. Added to these difficulties is this, that colour-change in *Hippolyte* is no simple reflex affair taking place "with the certainty of a physical experiment," but is one subject to what, in times of difficulty, seems to amount to wilful perversity. What we believe to be the chief element in this seeming perversity is described under the head of Periodicity in Section VI.

III. *The Nature of the "Chromatophores."*

The colours of the pigments in the "chromatophores" determine the tint of the prawn by their disposition and the depth of its colour by their abundance. The "chromatophores" are by no means the simple, stellate, cellular, dermal structures which they are commonly supposed to be. One series of them lies under the epidermis, another is interspersed between the muscle fibres both of the great flexors and extensors of the tail and those of the appendages, while a third series—often forming great splashes of colour—invests the gut, nerve-cord, liver, and other internal organs.

In simple colour varieties—brown for instance—the pigment of the skin forms a dense network obscuring the "muscle-chromatophores." In such cases the colour of the prawn is determined by the colour of the superficial network. In other cases, when the prawns are banded or boldly barred, the colour-elements of the skin are absent or have no pigment, and the deep "chromatophores" alone determine the colour of the pattern. In many *Hippolyte* we have found that distribution of the pigment is the same in the skin-chromatophores and in the muscle-fibres which underlie them. The two sets are co-ordinated. This correspondence applies to other Crustacea, though it has not hitherto been recognised.

The pigments present in the chromatophores are limited to red, red and yellow, or red, yellow, and blue. These three may be present together in one and the same element. During colour-changes they are distributed independently of one another in the sense that one pigment may become aggregated in the centre of the "chromatophore," whilst another runs out into its network of processes. Change of colour appears to be due to a fresh pigmentary deal of the shuffled colour-pack.

Whether chemical changes play a part in converting one pigment to another or no, we are not yet in a position to say. All the evidence we have is against the view that the colour-elements of *Hippolyte* are cells like the chromatophores of the frog. The "muscle-chromatophores" bear tubular processes limited by a distinct membrane The processes of the skin-chromatophores penetrate between the epidermal cells and form networks. The movement of the pigment is not due to a change in the form of the "chromatophore" but to a movement flowing to or from the central part. Further details of these colour-elements are given in our larger paper

IV. *The Habits of* Hippolyte varians

Hippolyte lives in swarms amongst the weeds of the seashore. In some places it is most abundant upon the *Halidrys* and other algæ which flourish luxuriantly in the "laminarian" zone, and are only exposed by very low spring-tides. In other places the *Zostera*-beds and the masses of *Fucus* form its chief resorts. Each colour-variety is a marvel of protective coloration. Each is to be found among weeds of a closely similar hue and adheres to its chosen habitat with the greatest tenacity. Though it has the power of making powerful leaps and of swimming, only under the greatest provocation can *Hippolyte* be induced to take this exercise. At night as well as during the day these prawns are still to be found on their food-plants, and, should the receding tide lay the weeds bare, the *Hippolyte* may still be found by shaking them into a net Should the special food-plants of any given colour-varieties be mixed with other weeds, the prawns will after a time select, each after his kind, the weeds in which it naturally feeds and with which it agrees in colour Generally speaking, *Hippolyte* prefers shade to direct sunlight or to artificial light. The emerald green variety found on *Zostera*, whether at a comparatively high zone on the shore or in the "laminarian" zone, is exposed to a considerable amount of light on account of the "blades" of this grass being separated from each other and not growing in the shade of deep rock pools. The brown and red varieties of *Hippolyte* are, on the other hand, associated with dense masses of weed attached to rocks; so that the light-intensity in which they live, even at half-tide, is probably lower than that of the beds of *Zostera*. The bearing of these facts on the changes of colour are referred to in Section VIII.

Hippolyte exhibits a certain sexual dimorphism both with regard to size and to colour. This may be expressed by saying that the males are on the average much smaller and less elaborately patterned than the females, which are more resourceful in adjusting their colour to their surroundings. From the point of view of "protection" this is what might be expected owing to the greater sluggishness of the female, which in turn is partly due to the large mass of eggs or developing zoeæ, which she almost invariably carries

V *The Nocturnal Colour* "*Nocturnes.*"

Whatever the diurnal colour of *Hippolyte* may be, it changes at or soon after night-fall to a wonderfully beautiful transparent blue or greenish-blue colour. Prawns in this condition we designate as Nocturnes The depth of the nocturnal tint is directly proportioned to that of the diurnal colour, dark brown prawns becoming deep blue and light ones pale blue. Under natural conditions the nocturnal colour persists until daybreak. At the first touch of dawn the colour vanishes and that of the preceding day re-appears. Specimens trawled at night and in the early morning before daylight show that the nocturnal colour is perfectly normal and is assumed by *Hippolyte varians* while still on its food plant. Other Crustacea too, show a peculiar nocturnal colour. *Mysis*, for example, of different species and possibly even of distinct genera, show a transparent and blue colour-phase at night, giving place during the day to a deeply pigmented condition

Nocturnes are remarkable chiefly but not solely for their peculiar pigmentary condition. The red and yellow pigments are maximally contracted, while the blue is present in a very diffuse homogeneous condition forming a network which traverses the connective tissue of all the chief organs, particularly the muscles The peculiar transparency which accompanies this nocturnal condition is, however, not entirely explicable by the retraction of the red and yellow-coloured pigment. It is only one of a number of profound changes affecting the body as a whole Indeed we are prepared to say that the nocturnal state opens up a new chapter in biological investigation, and that by a study of this condition increased knowledge of the succession of metabolic processes may be gained.

VI *Periodicity of Colour-change.*

Under normal conditions *Hippolyte varians* passes through a daily colour-cycle. Its diurnal colour gives place to a slight increase of reddishness—a sunset-glow—just before night-fall, and this ushers in the nocturnal phase. These changes are periodic in the strict sense of the word. Though often modified by external agents they exhibit a certain independence of them In constant darkness a Nocturne recovers its diurnal colour. In constant light (of certain kinds at least) a diurnal form passes over to the nightly phase. Though light often induces and induces with marvellous rapidity a recovery from the nocturnal phase, it is often powerless to overcome the habit of the animal The periodicity is only slowly worn down in the course of two or three days. These changes express a nervous rhythm; perhaps a profound and rhythmic course of metabolic events. The reddish phase antecedent to the full nocturnal tint probably explains the statement made by M Malard,* that in darkness *Hippolyte varians* becomes red.

Periodicity is manifested in the colour-change of *Hippolyte* which have been deprived of both their eyes. The assumption of, and recovery from, the nocturnal phase is still effected, but more slowly and erratically than in normal specimens

VII *Range of Colour-change.*

Adult animals when placed with weed of a new colour (the light-intensity being as far as possible unaltered) are, under the conditions of the laboratory, only capable of very slow sympathetic colour-changes Thus green *Hippolyte* placed on brown weed conserve their greenness for a week or more, but in the end give way and become brown. Their subsequent recovery when placed with green weed is more rapid. We have repeated such experiments in the open time after time, and have found that the prawns were either quite refractory or responded in this slow manner. Yet these same specimens undergo the changes preceding and culminating in the nocturnal colour and the succeeding recovery to their diurnal tint, with the utmost readiness. The fact that prawns, refractory to sympathetic colour-change can and do undergo a rapid change of tint when the light-intensity or the quality of the light is altered, is shown by such records as the following· A specimen, one of a large catch, incidentally observed to be the blackest we have ever seen, became in a few minutes transparent when put in a white porcelain dish. Further, a ready and almost infallible means of producing transparent green *Hippolyte* and even a colour hard to distinguish from the nocturnal tint, consists in placing freshly caught prawns in a white porcelain dish, and covering the top with a piece of muslin. Under these circumstances the change often takes place very rapidly (thirty seconds to one minute).

It is therefore necessary to distinguish at least three kinds of colour-change in *Hippolyte varians.* First, the passage from the diurnal to the nocturnal colour-phase followed by recovery to the colour of the previous day. In this case the phases form a rhythmic daily cycle. Second, the colour-changes produced by artificially altering the light-intensity to which the prawns are exposed, or by subjecting them to light reflected from white, and especially porcelain surfaces. Third, the sympathetic colour-change brought about by change in the colour of the surroundings.

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* 'Bulletin de la Société Philomathique de Paris,' sér. 8, vol. 4, 1892, p 28

The first of these is habitual or periodic, and may be quickly produced, towards evening, by a profound alteration of the light-intensity. Even the natural recovery from the nocturnal to the diurnal colour takes place rapidly with the dawn. The second change, as inexplicable in teleological terms as the first, is also rapid, often very rapid.

The third change is extremely slow. The prawn, in the acquirement of its adult colour, is guided and guided solely, so far as external circumstances are concerned, by light-intensity. In response to the conditions of light-intensity which prevail in its habitat, the prawn metabolises and distributes its pigments. But its pigmentary forces do not admit of ready mobilisation for purposes of defence, or at least they do not quickly obey a command to move. *Hippolyte* by its immobility has gradually grown into its surroundings and though, as for example, at night, its pigments may be readily aggregated, and a special nocturnal colour produced, yet this mobilising power is not utilised at all, or but very slowly, to redistribute the pigments, when the colour of the habitat is changed.

VIII. *The Causes of Change of Colour*

(*a*) *Hippolyte* grows into harmony with its surroundings. So developed it hangs on to wave-swept weeds. Should it be dislodged its hope of concealment lies rather in a rapid choice of a weed of its own colour, than in a slow sympathetic colour-change on its own part, for if we may trust our experimental results, a week would elapse before the change could be complete. Monochromatic light (obtained by the use of Landolt's colour-filters), is singularly inefficacious in producing any sympathetic colour-change. Red, yellow, green, and blue light act in this respect like darkness. Under natural conditions we conclude, therefore, that the ultimate colour-change is effected by a reaction to light-intensity.

(*b*) After much trouble and many experiments, we find that there is no evidence that rays of light, by virtue of their specific wave-lengths, play any part in changing the colour of the prawns. On the other hand we find that in the diurnal phase, a low light-intensity favours expansion of the red pigment and so brown effects, while increased light-intensity produces a green tint. The appreciation of light-intensity appears to be very acute and to be the chief agent in producing colour-change.

(*c*) By ablation of the eyes, electrical stimuli, and heat, colour-changes may be induced. These agents have been employed in tracing the nervous mechanism of the change, but the histological examination of these experiments is not yet complete.

IX. *The Rôle of the Eye and Nervous System.*

Removal of one eye produces no effect on the body-colour. Removal of both, either produces no effect or a rapid nocturning. The animal is plunged in night. Under such circumstances the periodic habit may re-assert itself and a recovery with subsequent fairly punctual nocturning take place The chromatophores of detached limbs, and in the bodies of blinded specimens, exhibit alterations of their pigments—when subjected to different light-stimuli, quite similar to those which occur in the intact animal under the same conditions. Therefore (1) an intrinsic rhythmic nervous change supervises the periodic change in *the pigments*; (2) *the eye is the most important auxiliary in modifying* nervous control, (3) local government plays a part In variegated colour-forms, which show, as in a mirror, the pattern of their weed, it can scarcely be doubted that both central and local government co-operate, and so produce a result of such consummate delicacy. Here there is expansion of one pigment, here of another, there complete contraction. The light acting through the eye on the central nervous system cannot be supposed to differentiate itself into such diverse stimuli as are required to produce the colour-variety. Local control under a strong central organisation seems to be the only likely force and the evidence favours this view

X. "*Chromatophores*" *of larval forms.*

We have succeeded in hatching out the zoeæ of *Hippolyte varians* and in following their development for a short time. Several of the colour-elements acquire their pigment before the time of hatching. There are two pigments, one red and the other yellow by reflected and dull green by transmitted light. A few chromatophores contain red only. All the colour-elements are distributed symmetrically. They occur in the neighbourhood of the eyes, the liver, and at the sides of the abdominal segments.

In the zoea the pigments react with astonishing rapidity to certain changes of light-intensity. A bright light brings about very rapid contraction, while a dark background effects expansion of the yellow-green and more slowly a similar change in the red. A diffuse blue substance was noticed frequently as though exuding from the dense red body of some "chromatophores." This may be the result of a destructive action of light upon one or both of the pigments, and should this be so, we may find in a study of the larval stage the meaning of the blue colour of Nocturnes.

"An Experimental Research on some Standards of Light." By J. E. Petavel. Communicated by Lord Rayleigh, F.R.S.

Received July 31,—Read November 16, 1899.

The standards of light may be divided into two main divisions, viz.:—

1. Flame standards.
2. Incandescent standards.

The first class comprises such standards as depend for their constancy on the rate at which chemical combination is going on. Almost all the standards in actual use come under this division. The British candle, the Methven standard,* the Harcourt† pentane standard, the Hefner-Alteneck‡ amyl acetate lamp, the Carcel lamp, and the acetylene§ burner are among the best known.

Apart from the large number of independent investigators who have carried out researches as to the relative merits of these sources of light, reference may be made to the reports of several committees which have been appointed in this and other countries to investigate the subject.‖

The general conclusions reached may be fairly summed up by saying that the pentane gas standard and the amyl acetate lamp are the lights which, from every point of view, have been found most satisfactory. Of the two, the Hefner-Alteneck lamp is the better known, and has been the subject of the more complete experimental study; it may be taken as fairly representative of its class. The light emitted, as in the case of all the other flame standards, is seriously affected by atmospheric impurities. Liebenthal¶ has shown that, if e represent the quantity of water vapour in litres per cubic metre of air, the light L sent out will vary according to the formula—

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* 'Journal of Gas Lighting,' vol. 40, p. 42, 1882.

† See 'Brit. Assoc. Proc.,' 1877, p. 51, and 1898, p. 845; *also* 'Report of the Standard of Light Committees,' as below.

‡ 'Elektrotechnische Zeitschrift,' vol. 5, p. 20, 1884; also 'Electrical Review,' vol. 42, p. 759, 1898.

§ Proposed by Violle, Ch. Féry and Fessenden (see 'Comptes Rendus,' vol. 122, p. 79, 1896; also 'Comptes Rendus,' vol. 126, p. 1192, 1898).

‖ See Blondel's Report to the Congrès International des Electriciens at Geneva, 1896; Report of the Standard of Light Committee's meeting of the Institute of Gas Engineers, May, 1895, 'Journ. of Gas Lighting,' vol. 65, p. 1007, 1895; Report of the Standards of Light Committee to the British Association, 1888, p. 39; Dibden's Report to the Metropolitan Board of Works, 1885; Gas Institute Committee, 1884, and Board of Trade Committee; Preliminary Report of the Sub-committee of the American Institute of Electrical Engineers, 'Transactions,' vol. 13, p. 135, 1896; Rapport der Photometrie Com. der Vereeniging van Gasfabrikanten in Nederland, 'Journ. für Gasbeleuchtung und Wasserversorgung,' vol. 37, p. 613, 1894.

¶ 'Elektrotechnische Zeitschrift,' 1895, vol. 16, p. 655.

$$L = 1·049 - 0·0055r$$

This will cause a variation of about 4 per cent from one season of the year to another. The variations due to this cause are stated to be still more marked in the Harcourt and Carcel lamps.

Again, if r_1 represent the quantity of carbon dioxide present in the atmosphere, measured in litres per cubic metre, the light will be given by the formula—

$$L = 1·012 - 0·0072r_1$$

Variations in the height of the flame are of the greatest importance. If h is the height—

$$L = 1 + 0·025\,(h - 40),$$

or

$$L = 1 - 0·030\,(40 - h),$$

according as h is above or below 40 mm.* The mean variation is, therefore, over 2½ per cent per mm. Owing to the bright halo which surrounds the flame, it is by no means easy to adjust the height correctly.

Finally, although it was at first stated that the degree of purity of the amyl acetate had no very considerable influence on the light, this has of late been denied, some authorities going so far as to state that sufficiently pure amyl acetate cannot be obtained in France.†

These facts will suffice to show that the variations are mainly due to causes inherent in this class of standard. Some of the difficulties can be obviated by providing a chemically pure atmosphere, and researches are being carried out in America in this direction, but it is obvious that any such improvement will involve a considerable complication of the apparatus.

Incandescent Standards

In the case of standards of this class, the constancy of the light depends essentially on the constancy of the temperature at which the radiating body is kept, and on the constancy of the emissivity of the body at that temperature.

The temperature may be fixed by some definite physical phenomena as in the Blondel and Violle standards, or it may be determined in a more or less arbitrary manner, as in the Lummer and Kurlbaum standard.

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* 'Journ. fur Gasbeleuchtung und Wasserversorgung,' vol. 31, p. 583, 1888, or 'Elektrotechnische Zeitschrift,' vol. 9, p. 96, 1888.

† 'Rapport sur les Unités Photométriques,' A. Blondel, Congrès International des Electriciens, Genève, 1896.

Any solid substance that would not disintegrate at a temperature of about 1700° C. might, *à priori*, be chosen as a radiator It has, however, been shown that most of the oxides when maintained at these high temperatures undergo a change in their emissive properties, and cannot, therefore, be used for the purpose we have in view [*] The choice thus seems restricted in practice either to carbon or to one of the metals of the platinum group

Before passing on to the experimental part of the work, it may be well to recapitulate the necessary qualifications of a standard of light The requirements may be briefly summed up under three heads —

1 The standard must remain constant.

The slow periodic variations of the amyl acetate lamp which extend over a period of several months are as much to be avoided as the flickerings of the candle or the hourly changes of the Carcel lamp

2 The standard should be reproducible

This condition is satisfied when the standards reconstituted by independent investigators show no measurable variation between one another

3. The light emitted should be as nearly as possible of the same spectral composition as that of the chief artificial lights now in common use [†]

On the Intrinsic Brilliancy of the Crater of the Arc

For many years it had been noticed that the area of the crater of an electric arc when burning between carbon poles increased about in proportion to the current, also that the light emitted increased in the same ratio as the area of the crater [‡]. These and other facts led to the conclusion that the temperature of the crater remained constant The hypothesis was put forward that this temperature was the boiling point of carbon, this theory being supported by the experiments made in 1892 by J Violle.[§] In the same year it was proposed simultaneously by Swinburne, S P Thompson, and Blondel,[||] that the crater of the arc should be used as a standard of light, Blondel publishing a series of experiments illustrating the way in which the new standard might be used

In 1894 A Trotter[¶] proved that when the arc is not silent the crater is formed by a point or line of very high intrinsic brilliancy rotating at a speed of from 50 to 450 revolutions per second.

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[*] Nichols and Crehore, 'Trans of the Amer Inst. of Electrical Eng,' vol 13, p 190, 1896

[†] Strictly speaking, it is only when two lights are of the same spectral composition that the ratio of their intensities can be expressed by a single figure

[‡] Professor S P Thompson's Cantor Lectures, 1895, see also "The Electric Arc," by Mrs Ayrton, 'The Electrician," vol 34, p 399, 1895

[§] 'Journ de Physique,' 3 sér, vol 2, p 545, 1893, and 'Comptes Rendus,' 1892, p. 1274

[||] See 'Proc. of the Int. Electrical Congress at Chicago,' 1893, pp. 259, 267, 315.

[¶] 'Roy Soc. Proc,' 1894, vol. 56, p 262; see also "Effect of Pressure on the Temperature of the Arc," E Wilson and G. F Fitzgerald, 'Roy Soc Proc.,'

The existence of the above mentioned phenomenon forms a serious objection to the use of the electric arc as a primary standard, but it does not, *per se*, render its use impossible.* Finally, the variation of the intrinsic brilliancy of the crater is a question which in itself offers considerable interest.

The points on which the present work bears are threefold—

1. What is the average intrinsic brilliancy of a normal (silent) arc?
2. When the conditions are carefully specified, are the variations still too great to allow of the use of this source of light as a standard?
3. What variations can be obtained by the use of excessive currents and current densities, and by surrounding the arc with an enclosure maintained at a very high temperature?

To obtain consistent results the observations must be made on a very small area selected from the central portion of the crater.

The diaphragm used for this purpose is shown in fig. 3. The opening, *d*, is 1·47 sq. mm. in cross-section.† The diaphragm is shaped much like the tuyere of a blast furnace, and being kept cool by a water circulation it can be placed within a very short distance of the crater of the arc.

The next question involved was the determination of the best relative position of the carbons. In the usual arrangement of the arc the centre of the crater is hidden by the negative carbon. A modification of this arrangement, used by Blondel, which consists in slanting the carbons and placing the positive slightly behind the negative, was found not to be entirely satisfactory. An attempt was made to take the observations through a hole drilled out of the negative carbon (see fig. 1), but when the arc was started this hole became filled with mist, and the plan had to be given up.

Fig. 2 shows diagrammatically the next arrangement which was tried. The carbons n_1, n_2, n_3, are negative, and form the edges of an equilateral pyramid, the axis of which is in the prolongation of the positive carbon P. The summit of the pyramid is at the point P. The crater formed on the positive carbon by these three arcs is in

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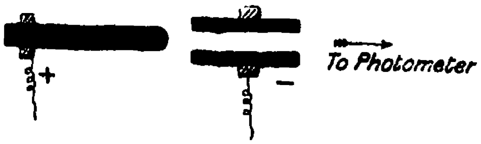
vol. 58, p. 174, 1895; vol. 60, p. 377, 1897; see also the account of the discussion on this subject on the 19th February, 1897, at the Société Française de Physique.

* On this subject see Captain Abney, 'Journ. of the Inst. Elec. Eng.,' vol. 28, p. 443, 1899.

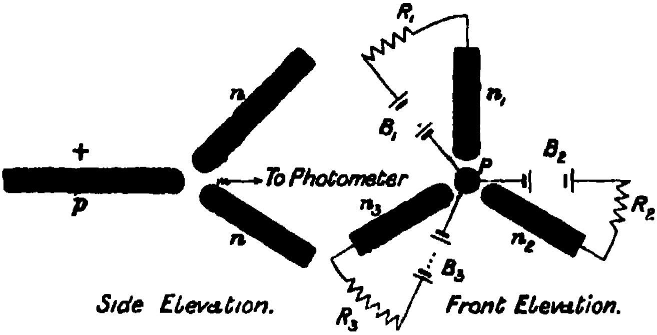
† It has been shown by Professor S. P. Thompson (see 'Phil. Mag.,' vol. 37, p. 120, 1893) that when diaphragms of very small diameter are used the thickness of the plate in which the aperture is pierced introduces a serious error in the measurements. This difficulty was avoided by counter-sinking the opening.

FIG. 1.



full view, and good results might have been obtained in this manner Unfortunately this disposition had to be abandoned, as it soon became evident that it was impossible for one observer to adjust the currents in the three independent electrical circuits, to feed up the four carbons, and to make all the electrical and photometric readings

FIG 2 —P, positive pole; n_1, n_2, n_3, negative poles; R_1, R_2, R_3, variable resistances; B_1, B_2, B_3, batteries.



The disposition which was finally adopted is shown in fig. 3 The diaphragm *d* screws into a screen *s* This screen is supported on a system of pivots and levelling screws, so that it can be raised, lowered, or turned round a vertical or horizontal axis. The opening *d* can thus be directed to any portion of the crater The positive carbon is horizontal, and so placed that its axis coincides with the axis of the photometer.

Two distinct series of experiments were carried out: one with the arc placed in a metallic enclosure kept at about 20° C. by a water circulation, the other in the enclosure shown in fig. 3. The temperature of this enclosure varied from the melting point of silver to near the melting point of platinum, according to the amount of power expended in the arc. Referring to fig. 3, *c* is a carbon crucible surrounded by a thick layer, *b*, of firebricks and refractory clay. The outer covering *a* is of asbestos. Both the high and low temperature enclosures were provided with a small camera obscura (not shown in the

FIG. 3 —*a*, asbestos box; *b*, lining of refractory bricks; *c*, graphite crucible; *d*, diaphragm, *s*, screen

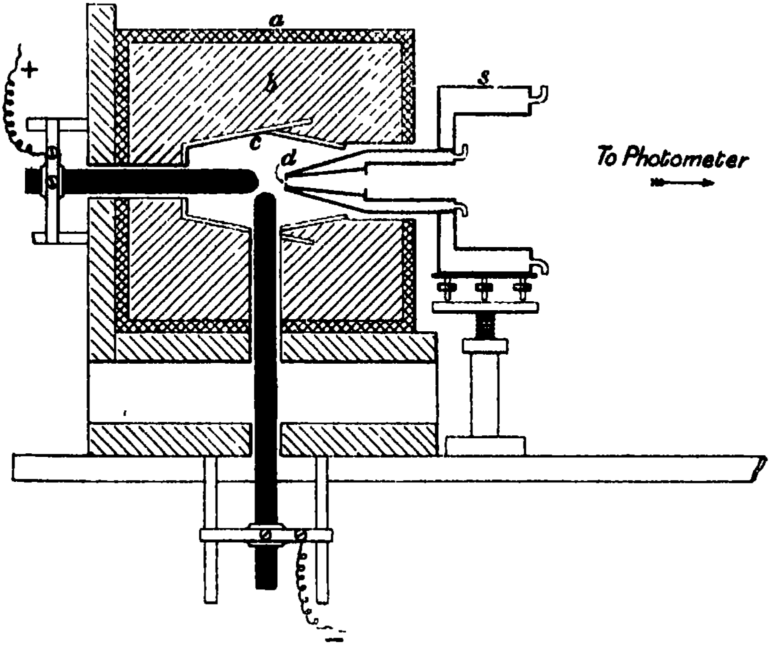


figure), by aid of which the relative positions of the carbons could be adjusted and the length of the arc measured

"Apostle" carbons were used in all the experiments The size of the positive carbon varied from 6 to 25 mm in diameter

To obtain reliable results a sufficient current density must be used to give a fairly large crater The arc must also be sufficiently stable for the crater to remain some considerable time without shifting its position Finally, it is desirable that the arc should be burning in a normal manner, and therefore neither hissing nor roaring

Given these conditions, it is impossible to vary either the current, the electromotive force, or the length of arc, within very wide limits

In the experiments recorded in Table I, the mean intrinsic brilliancy is 147 candle power per square millimetre, the variations from the mean amounted to 10 per cent

Table II gives experiments made under similar conditions, but with the enclosure in which the arc was burning at a temperature of over 900° C. The average intrinsic brilliancy calculated from this table is 143, or about 3 per cent lower than when the enclosure was at the ordinary temperature It would, however, be unwise to attach too much importance to this change. The difficulty of obtaining consistent

Table I.—"Silent" Arcs. Enclosure at about 20° C.

Electromotive force in volts	Current in amperes	Power in watts	Diameter of the positive carbon in mm	Current density in amperes per sq mm of cross-sectional area of positive carbon	Length of arc in mm	Intrinsic brilliancy of the crater in candle-power per sq mm [1]
65 0	8·1	526	8	0 161	4 8	136
64 5	7 0	452	8	0 130		141
72·0	10·3	742	8	0 205	6 0	143
70 3	7 3	513	6	0·258		147
61 7	6 0	370	6	0 212	5 1	154
62·0	9 1	564	6	0 322	6 0	160

[1] Each of the figures in this column is the mean of a number of photometric observations.

Table II.—"Silent" Arcs. Enclosure above 900° C.

Electromotive force in volts.	Current in amperes	Power in watts	Diameter of the positive carbon in mm	Current density in amperes per sq mm of cross-sectional area of positive carbon.	Length of arc in mm	Intrinsic brilliancy of the crater in candle-power per sq mm [1]
88·0	11·0	913	8	0·219	.	136
57·0	10 9	621	15	0·061	.	142
61 0	8 6	525	8	0 171	6·0	152

[1] Each of the figures in this column is the mean of a number of photometric observations

results is considerable, and a 3 per cent. variation is well within the experimental error.

The conclusions we have reached may be summed up as follows:—

1. The intrinsic brilliancy of the crater of a silent arc is about 147 candle power per square millimetre.

2. Even when the most favourable conditions are selected, and the intensity of current and the length of the arc are maintained constant, it is difficult to obtain consistent results, variations of over 5 per cent. being by no means unfrequent. The crater of the arc does not, therefore, possess the qualities required of a standard.

3. Variations in the size of the carbons, in the intensity and density of the current, in the length of the arc, and in the total power expended (as long as the arc is kept silent), will not cause the intrinsic brilliancy to vary more than 10 per cent on either side of the mean

4. No sensible variation in the intrinsic brilliancy, and therefore in the temperature of the crater, is produced by placing the carbons in an enclosure maintained at over 900° C.

With regard to the constancy of the temperature of the crater, these results are not without importance

Having this question in view, it was necessary to determine what were the effects of extreme variations of current density and power

In Tables III and IV will be found the results of observations taken when the arc was hissing *

Table III.—"Hissing" Arcs Enclosure at 20° C

Electro-motive force in volts.	Current in amperes	Power in watts.	Diameter of the positive carbon in mm.	Current density in amperes per sq mm of cross-sectional area of the positive carbon.	Length of arc in mm.	Intrinsic brilliancy of the crater in candle-power per sq. mm.[1]
70·0	15·8	1106	8	0·314	4·8	136
42·0	21·6	907	8	0·410	1·2	143
53·0	25·8	1367	8	0·514	2·9	157
44·0	50·0	2200	15	0·283	..	160

[1] Each of the figures in this column is the mean of a number of photometric observations.

It will be seen that the current varied from 6 to 50 amperes, the current density from 0·03 to 0·51 ampere per square millimetre, and the power from 370 to 2800 watts.

The lowest photometric readings gave 119, and the highest 160 candle power per square millimetre.

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* The word "hissing" is used here as being the generally accepted term. It is only when the current density is small that it is actually descriptive of the sound made, as the current increases, the pitch rises, until with a very short arc and a current density of about 1 ampere per square millimetre, the sound is between a whistle and a scream The arc then assumes a very peculiar aspect, a pointed blue flame, like the flame of a blow-pipe, being sent out from the crater. This effect was most marked when the high temperature enclosure was used. With the above current density the entire carbon becomes white hot and burns away with great rapidity, an increase in the intrinsic brilliancy seems also to take place Unfortunately, it was not found possible under these circumstances to obtain reliable photometric observations.

Table IV.—"Hissing" Arcs. Enclosure above 900° C

Electromotive force in volts	Current in amperes	Power in watts.	Diameter of the positive carbon in mm.	Current density in amperes per sq. mm of cross-sectional area of the positive carbon	Intrinsic brilliancy of the crater in candle-power per sq mm[1]
79·0	18·0	1422	15	0·102	119
45·6	42·0	1915	15	0·238	121
60·3	18·6	1122	25	0·038	130
52·0	55·0	2860	25	0·112	133
41·0	43·4	1779	25	0·088	137
50·0	39·3	1965	15	0·222	142

[1] Each of the figures in this column is the mean of a number of photometric observations

If we assume that the formula *

$$t - 400 = 889{\cdot}6\sqrt[6{\cdot}9]{b}$$

(t = temperature in degrees centigrade, b = intrinsic brilliancy in candle power per square centimetre), holds good for carbon at these high temperatures, the above change in candle power corresponds to a variation of temperature of from 3866° to 4018° C The total alteration in absolute temperature thus works out at 4 per cent. Observations made on silent arcs (Tables I and II), reduced in the same manner, give the extreme limits of temperature as 3935° and 4018° C, or a change of 2 per cent. in the absolute temperature.

These variations are somewhat greater than we should meet with in the case of substances boiling at ordinary temperatures. It must, however, be borne in mind, that even in the case of a silent arc, the crater is the seat of several secondary phenomena, which under certain circumstances may affect the boiling point.† The term boiling point is in itself misleading, as it seems possible, if not probable, that at atmospheric pressure carbon does not become liquid, but like carbon dioxide, passes direct from the solid to the gaseous state. It has frequently been stated that impurities cannot affect the temperature of the crater, as all known bodies become gaseous at a lower temperature The experimental data to substantiate this are entirely wanting; silica, lime, alumina, magnesia, and other substances are solid at the tem-

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* 'Phil. Trans.,' vol. 191, A, p. 515, 1898.

† Mrs Ayrton, "On the Hissing of the Electric Arc," 'Journ. of the Inst of Elec. Eng.,' vol. 28, p. 401, 1899; *also* Dr. J. A. Fleming's *remarks during the discussion of Mrs. Ayrton's paper*, p. 439.

perature of melting platinum, and it is difficult to predict at what temperature they volatilise.

Taking everything into consideration, it may therefore be said that the present experiments confirm the theory that the crater of the arc is at the temperature of volatilisation of carbon

The earliest determinations of the intrinsic brilliancy of the crater were made in 1878 at Chatham under the direction of the Royal Engineers' Committee * The results varied from 39 to 442 candle power per square millimetre, the mean value being 110. With regard to more recent researches, Trotter gives the intrinsic brilliancy as 64, Weber as 70, and Blondel as 158 candle power per square millimetre. With the exception of those of Blondel all previous results are very much lower than the values I have obtained The discrepancy may be attributed to the fact that instead of using a diaphragm to select the rays from the centre of the crater, the first named observers estimated the total area of the crater, and compared it with the total amount of light.

On the Lummer and Kurlbaum Incandescent Platinum Standard.

In 1894 Drs O. Lummer and F Kurlbaum proposed a new standard of light.† A strip of platinum foil 25 mm. wide, 0 015 mm. thick is brought to incandescence by an electric current of about 80 amperes. The temperature is increased until one-tenth of the total radiation is transmitted through a water trough 2 cm. in width. This ratio is determined by means of a bolometer. The construction of the instruments require the greatest care, and three months had passed before I was able to obtain the first observations The instruments used for this work are shown in fig. 4. The explanation appended to this drawing is sufficient to render further description unnecessary.

It would be useless to give details of the experiments which in the main confirm the results already obtained by Lummer and Kurlbaum.

With the same apparatus used in the same manner the light is practically constant as long as the ratio of radiations is adjusted to 1/10 per cent.

The adjustment of the temperature of the platinum foil with the required degree of precision is, however, most tedious, and in fact all but impossible, except under the most favourable conditions. This consideration, together with the complicated nature of the apparatus, render this light impracticable as a working standard.

At the beginning of the present paper it has been pointed out that a

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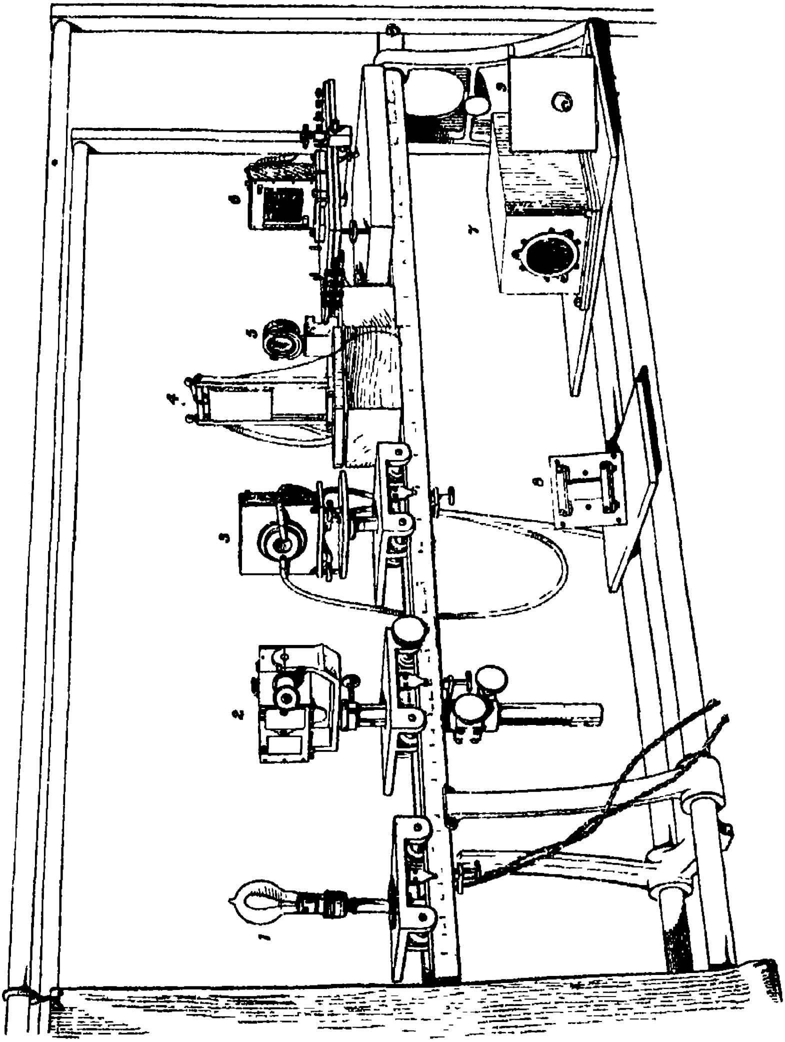
* R E. Committee extracts for 1879; Report of the Electric Light Experiments carried out at the School of Military Engineering at Chatham.

† 'Berichte der Preuss. Akademie,' 1894, p. 227; 'Elektrotechnische Zeitschrift,' 1894, p. 475.



IG. 4.—Apparatus used for the investigation on the Lummer and Kurlbaum incandescent platinum standard:—

1. Incandescent lamp serving as standard of reference.
2. Lummer and Brodhun photometer head.
3. Enclosure in which the incandescent platinum foil is placed with the 1 sq. cm. diaphragm.
4. Screen to cut off the radiation from the bolometer.
5. Water-trough, with quartz sides 2 cm. apart.
6. Bolometer with the cover removed to show the films.
7. Bolometer cover.
8. Clips for holding the incandescent platinum foil; this plate fits on to the back of the enclosure 3.
9. One of the water circulation diaphragms used to keep any extraneous radiation from the bolometer.

standard must be constant, reproducible, and must emit light of a suitable colour. We have just seen that the source of light under consideration may be regarded as fulfilling the first of these conditions, with regard to the second and third, the conclusions are less favourable.

By definition, the ratio between the total radiation and that transmitted through the water trough must be as ten to one, 1 per cent error in the electrical measurements causing 3 per cent. change in the light. To obtain the required degree of precision the galvanometer deflections must be reduced to equality. Three methods are available :—

1. The bolometer current may be reduced in the ratio of one to ten
2. A 1/10th shunt can be introduced into the galvanometer circuit.
3. The distance between the bolometer and the radiator may be varied in the ratio of 1 to $\sqrt{10}$.

None of these methods is entirely satisfactory. The reduction of the current through the bolometer involves a considerable complication of the apparatus, and interferes with the steady working of the instrument. It is also by no means impossible that the resulting change in the temperature of the bolometer films may change their coefficient of absorption. With a shunted galvanometer, temperature changes and thermo-currents are a frequent source of error. The most reliable method is to change the distance of the radiator; but here again two objections arise. The enclosure containing the platinum foil is rarely at a temperature so closely approaching that of the bolometer films that shifting this large surface leaves the instrument unaffected. The law of inverse squares cannot strictly be applied, as the rays of light are refracted when passing through the water trough. In practice, marked variations were obtained when the method of determining the ratio of the two radiations was altered.

The use of another form of bolometer produced a considerable change in the light emitted. The bolometer films are coated with platinum black by electrolysis. The composition of the bath, the electromotive force, and the current have been specified; but it was found that the temperature of the bath and the resistance of the film affected the nature of the deposit obtained

Finally, the spectral composition of the light is unsatisfactory, the colour being much too red.

We are thus driven to the conclusion that this light does not possess all the qualities required of a primary standard. Under certain circumstances it may be of the greatest value as a standard of reference. It is used in the Reichsanstalt to check the values of the Hefner lamps; for purposes of this kind it is well suited, and might with advantage be more frequently used.

Preliminary Research on the Molten Platinum Standard of Light.

The use of molten platinum as a standard of light was first proposed by J. Violle* at the Congrès International des Electriciens on the 21st of September, 1881.† In 1884 Violle published an account of his experiments with the new standard; which was adopted in the same year by the Conférence Internationale pour la Détermination des Unités Electriques on the 2nd of May, 1884.‡ The adoption was confirmed by the Congrès International des Electriciens on the 21st of May, 1889, when it was specified that the practical unit should be the *bougie décimale*, and by the International Congress in Chicago in 1893, and the Congrès International des Electriciens at Geneva in 1896, when the use of the Hefner lamp as a practical standard was recommended.

Since the adoption of this unit the aim of most of the experimental work on the subject has been not so much to find a suitable method of using the Violle standard as to obtain a substitute for it. These efforts have resulted in the well-known instrument devised by W. Siemens§ for fusing platinum foil, and in the proposal made by C. R. Cross‖ to use as a practical standard the light emitted by a thin platinum wire at its melting point.

Physiological considerations render such a thing as an instantaneous photometric reading an impossibility, and herein lies the principal reason why the above proposals have had to be abandoned. Apart from this fact, it is extremely doubtful if either in the case of fine wire or thin foil the break occurs actually at the temperature of fusion of the metal.¶ Cross admits that there were considerable differences in the quantity of light emitted per unit area when the diameter of the wire was changed. In connection with the present work a number of experiments were tried, both with wire and foil, but the results were by no means encouraging.

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* For Violle's researches see 'Comptes Rendus,' vol. 88, p. 171, 1879, vol. 85, p. 543, 1879, vol. 92, p. 866, 1881; 'Lumière Electrique,' vol. 14, p. 475, 1884, 'Annales de Chimie et de Physique,' sér. 6, vol. 3, p. 373, 1884.

† 'Comptes Rendus du Congrès International des Electriciens,' Paris, 1881. Assemblée générale, Troisième Séance, p. 50. See also 'Procès-Verbaux de la Conférence Internationale pour la Détermination des Unités Electriques.' Troisième Commission, Séance du 20 Octobre, 1882, p. 131.

‡ 'Conférence Internationale pour la Détermination des Unités Electriques.' Deuxième Session, Troisième Séance, Mai 2, 1884, pp. 23, 115. "L'unité de chaque lumière simple est la quantité de lumière de même espèce émise en direction normale par un centimètre carré de surface de platine fondu, à la température de solidification. L'unité pratique de lumière blanche est la quantité totale de lumière émise normalement par la même source."

§ 'Elektrotechnische Zeitschrift,' vol. 5, p. 244, 1884.

‖ 'Electrician,' vol. 17, p. 514, 1886.

¶ See 'British Association Fourth Report of the Standards of Light Committee,' App. II, 1888, p. 47.

As far as I am aware only one research has been carried out with a view of repeating the Violle standard on anything like the scale originally used. This research was made at the Reichsanstalt* in Berlin The work proved unsuccessful, and a detailed account of the experiments has never been published This renders it very difficult to suggest a reason why reliable results were not obtained.

In the case of molten platinum, owing to the high rate at which heat is being dissipated, it is quite possible for one part of the mass to be liquid while another part only a few millimetres distant is considerably below the temperature of solidification. This source of error should be minimised by the choice of suitable experimental conditions, and by reducing the results by some method similar to the one given below Again, all the magnesia or lime bricks I have been able to secure contain a certain proportion of silica. This, as will be pointed out later, is sufficient to render them useless for the purpose in view. In the account of the Reichsanstalt experiments this difficulty is not mentioned

In common with all other pure substances, platinum has a constant freezing point, the length of time, however, during which the constancy of temperature will be maintained, depends mainly on three factors

If we take D to represent the quantity of heat dissipated by the platinum per unit time when at its temperature of solidification, H the heat supplied to the metal per unit time, and L the total latent heat of the mass, and supposing for the present that the thermal conductivity is very great, the time during which the temperature will remain constant will be—

$$t = \frac{L}{D - H}.$$

For the object we have in view the time of constancy must be made as great as possible

One method of increasing the time t is to make H very nearly equal to D, or, in other words, to supply heat to the molten metal at nearly the same rate as the metal is losing it. We may supply the necessary quantity of heat H by means of an electric current, or, using a large surface of metal, we may keep the blow-pipe going on one part of the surface while the observations are being made on some other portion

If we abandon the idea of supplying heat during the time the observations are being taken, the formula becomes—

$$t = \frac{L}{D}.$$

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* 'Thätigkeitsberichte der Reichsanstalt,' 1890 and 1892—1894, or 'Zeitschrift fur Instrumentenkunde,' vol. 11, p. 149, 1891, and vol. 14, p. 266, 1894.

In order to reduce the amount of heat dissipated we must place the platinum in an enclosure at nearly the same temperature as the metal itself, take the observations through a relatively small opening, and make the thermal insulation of the mass as perfect as possible In the above formulæ, L being a function of the volume and D a function of the surface, we may, for any given circumstances, increase the time t by increasing the total volume of the metal

But it now becomes necessary to take into account the thermal conductivity of the metal. Strictly speaking, any given portion of the surface can only be at the standard temperature t_s for the very small fraction of a second during which the solid film is forming, later on the surface is at some lower temperature $(t_s - u)$ where u is the temperature interval which is required to cause the heat to flow from the parts of the mass where the process of solidification is actually in progress to the surface at the same rate as it is there being dissipated. Thus by increasing the mass of the metal we cannot indefinitely increase the time available for the photometric observations Unless the rate at which the heat is being dissipated at the surface under observation is made small, the temperature will not remain constant during the process of solidification, but will fall more and more rapidly as a larger proportion of the mass becomes solid.

On the Fusion of Platinum by an Electric Current.

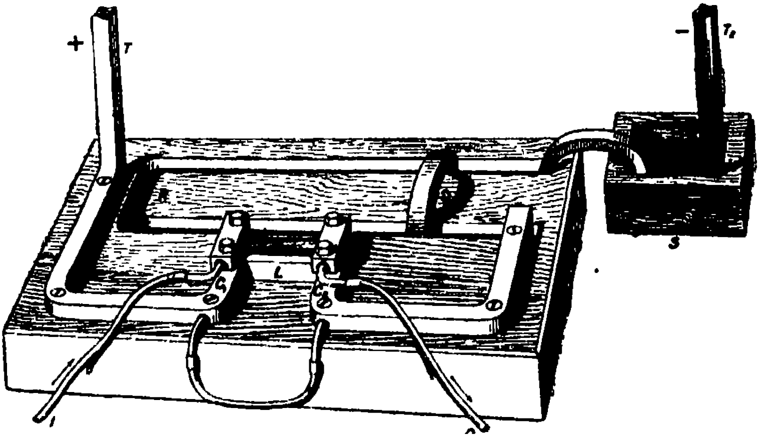
From the considerations that have just been enumerated, the advantages of an electric method of heating the metal are at once apparent.

In the preliminary experiments a number of different forms of electric furnace were tried In some cases the electric arc was used, in others the crucible was surrounded with a layer of graphite through which the heating current was passed. It is well known that at high temperatures platinum combines with carbon forming a carbide, and thus the use of graphite crucibles was out of the question

It is unnecessary to give further details as the experiments were not successful. No material could be found that would resist the high temperature of the arc and the chemical action of the incandescent carbon, and at the same time not affect the purity of the platinum.

It was then decided to fuse the platinum by passing the current directly through the metal itself. The apparatus shown in fig. 5 was designed for this purpose. Some forty large secondary cells were, by aid of the mercury switch, connected in parallel on to two "'bus-bars." The current was carried by massive copper leads to the terminal T_1, and from there by a one square inch bar to the clip C_1 passing through the platinum bar A to the clip C_2. R is a U-shaped mercury trough serving as a variable resistance. The copper short-circuiting piece D could be slid along the U so as to increase or diminish the effective length of the mercury. The clips C_1 and C_2 were hollow and kept cool by a water circulation. A rapid flow of water was also maintained over the surface of the mercury. The electric circuit was completed through the mercury switch S and an amperemeter.

FIG 5.—Apparatus for the fusion of platinum bars by an electric current. A, platinum bar, C_1, C_2, copper clips in which the platinum is held, L, lime trough which supports the fused metal; R, mercury trough serving as a variable resistance; D, copper short-circuiting piece, S, mercury switch, T_1, T_2, massive copper leads to the battery terminals, I and O, water inlet and outlet



With this arrangement currents up to 2000 amperes could be maintained for several hours. The platinum was supported by a trough, L, of some refractory material which was carefully cut to fit the metal bar. From the first it was evident that the principal difficulty would be to find a suitable material for this support. The weight of the molten metal acting on the supporting material gradually causes the shape of the trough to alter, and in a comparatively short time the cross-section of the metal becomes very irregular. The platinum then superheats in the place where the cross-sectional area has been most reduced, a spark occurs, and the molten metal thrown aside by the explosive force of the discharge, freezes before it has time to flow back into position. It is of course easy to re-weld the platinum, but the same difficulty will recur time after time and at more frequent intervals as the shape of the trough becomes more and more distorted. Some twenty bars of different shapes and sizes were used with the same result. Had it been possible to use larger bars, any small distortion of the trough would probably not have affected the experiments. However, as already stated, 2000 amperes was the maximum current available, and with this current it was found impossible to melt bars of much more than 70 sq mm in cross-section.

The length of the span between the edges of the clips was usually 70 mm., but in some cases this length was reduced to 40 mm

Lime, alumina and magnesia were the materials used for the troughs

It became evident that small platinum bars could not be maintained for any length of time in the molten state when supported on the ordinary refractory materials, and it was decided to make use of metal supports.

The question of cost excluded the use of the metals of the platinum group, and in default of a better material the trough was made of nickel. A number of nickel plates insulated from each other by mica were bolted together so as to form a solid block. In this block a cylindrical groove was cut, its axis being perpendicular to the direction of the laminations. The nickel plates were 1 8 mm in thickness, every alternate plate projecting 5 mm on the sides and 6 mm below. Through the channels thus formed a rapid circulation of water was maintained. The platinum bar was semi-circular in cross-section and deeply grooved on the lower side where it came in contact with the nickel The shape was much the same as would be obtained by cutting an ordinary bolt in two along its axis, the edges of the V-shaped thread bearing on the nickel. In this manner the somewhat anomalous experiment of fusing platinum in a nickel crucible was successfully carried out. Needless to say that the anomaly is only apparent, the molten platinum did not come in contact with the nickel, but was supported in a shell of solid platinum. The cooling was so efficacious that the nickel crucible never once became red hot. When the metal is fused in this manner the quantity of heat lost is very considerable, and the effective cross-section of the bars had to be greatly reduced The cross-sectional area, as measured from the bottom of the grooves, did not exceed 30 sq. mm. At the surface of the bar the central channel of molten metal was not more than 2 mm in width. If the current were forced so as to melt a larger surface, the bottom of the threads softened sufficiently to allow the fused platinum to run through, thus causing the bar to break.

Even when this occurred the platinum was found to freeze rapidly enough to prevent it alloying with the nickel support

If the above experiments were made on a larger scale some valuable results would certainly be obtained. By the use of a large welding transformer there should be no difficulty in fusing bars of ten or twenty times the cross-section of those used in the present instance. As will be seen below, good results can be obtained when the ordinary oxy-hydrogen flame is used to fuse the platinum, but, taking everything into account, the electrical method, if carried out on a sufficient scale, would not only be more simple, but would afford a more ready means of varying the rate of cooling

On the Fusion of Platinum by the Oxy-hydrogen Blow-pipe.

The object of the following researches was to repeat the work done some years ago by Violle, and to determine what are the best experimental conditions It is a somewhat curious fact that, although Violle makes no mention of having encountered any special difficulties, his experiments have never been repeated with success. Even under the best conditions (such as are obtainable, for instance, at the Reichsanstalt) the results have not proved satisfactory. Under these circumstances, it seemed hardly advisable to attempt any further research in this direction. But the theoretical advantages of the Violle standard appeal strongly to anyone who has studied the standards of light now in practical use, and it was thought that, whatever the final result, the time spent in this study would not be lost

The preliminary experiments occupied some considerable time, and nearly six months passed before the conditions under which the fusion should take place were clearly established. The platinum was fused several hundred times, the conditions being varied in every conceivable way. The shape of the furnace, the material of which it was made, the form of the blow-pipe, the relative proportion of the gases, in fact, everything that could have any bearing on the final result was in turn studied, and the most favourable conditions determined. The slightest impurity on the surface of the platinum has a considerable effect on the quantity of light emitted. When cold the surface always appears fairly bright and clean, but there is a temperature between a white and red heat at which the smallest impurity is clearly visible. At this temperature the platinum ingot, if it be quite pure, is very similar to a pool of molten glass. Any small impurity will cause a slight haze, resembling that produced on a mirror by a breath of moist air; if the impurities are present in a larger proportion the surface becomes very similar to a sheet of ground glass.

The necessary conditions to ensure a pure surface may be briefly summed up as follows:—

1. The platinum must be chemically pure.
2. The crucible must be made of pure lime.

Pure magnesia does not form a sufficiently coherent mass, and, alumina being light and flocculent, allows the metal to sink through it.

The lime should be entirely free from silica, one-quarter per cent. being sufficient to spoil the surface.

The best method of preparing the lime is to ignite calcium carbonate which has been precipitated from calcium nitrate by ammonium carbonate.*

3. The hydrogen burned must contain no hydrocarbons

At these high temperatures platinum combines readily with carbon, and any carbon in the flame would rapidly tarnish the surface of the metal.

4. The gases should be burnt in the ratio of four volumes of hydrogen to three of oxygen

All the best results were obtained when using the gases in the above-mentioned proportion. The temperature of the flame is then but little above the melting point of platinum, and the metal does not superheat to any great extent. When the platinum is considerably superheated it slowly distils, the drops condensing on the brick forming the cover of the furnace, the metal then drips back into the crucible carrying with it some of the silica which the brick contains, and thus contaminating the surface which is under observation An excess of oxygen in the flame is also favourable, inasmuch as it oxidises any impurity that the platinum may contain.

Finally it is important that the blow-pipe should be so constructed as to ensure a thorough mixture of the two gases.

The most favourable conditions being established by these preliminary experiments, the necessary apparatus had now to be designed. It may be thought that the instruments are unnecessarily complicated, but it must be borne in mind that a single experimenter had not only to make all the observations, but also to regulate the flow of the gases, the electromotive force on the terminals of the standard lamp, and the current of water passing through the diaphragms. Most of the apparatus had, therefore, to be made automatic in its action

The instruments serving to regulate the supply of gas are shown in fig. 6. The gas supply is stored in large cylinders at a pressure of from 100 to 200 atmospheres S is a device worked by an electric current from a key placed under the photometer head. When the key is pressed the chopper C falls, compressing the india-rubber tubes and cutting off the gas from the blow-pipe. (This device is shown in elevation at the bottom of fig. 6, on the left-hand side.) The hydrogen escapes through a valve to the burner B where it is ignited, the oxygen blows off through the water trap T When the chopper C is up the gases, after passing through the meters provided with scales reading to one-hundredth of a cubic foot are burnt from the blow-pipe P.

On one of the circuits the gas is forced through a diaphragm with an opening of about one-hundredth of the normal cross-section of the

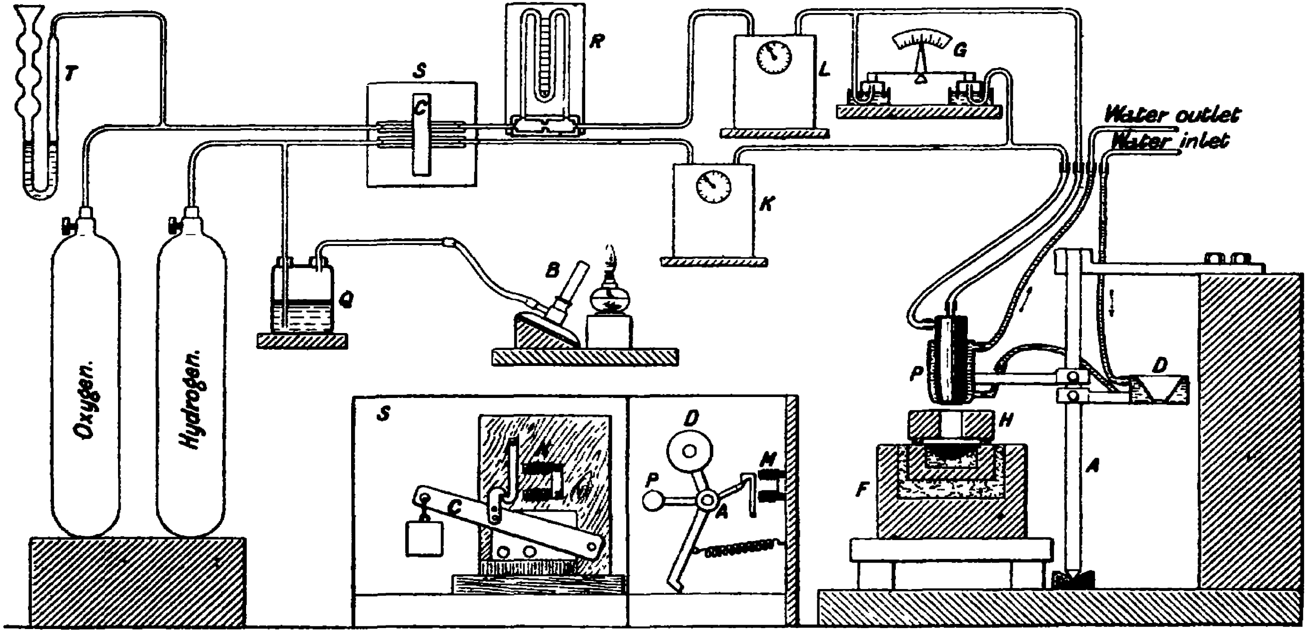
---

* This method was suggested by Dr. A. Scott, to whose kind advice the solution of many of the chemical problems involved in the present work is due.

FIG. 6.—Apparatus used for fusing platinum with the oxy-hydrogen blow-pipe. T, oxygen blow-off valve, Q, hydrogen blow-off valve; S, automatic device for cutting off the gas from the blow-pipe (seen also in elevation in small inset figure at bottom), R, "rate" gauge; L and K, oxygen and hydrogen meters; G, "ratio" gauge; P, blow-pipe, D, diaphragm, H, cover of furnace; F, furnace; A, axis which supports the blow-pipe and diaphragm; M and N, electro-magnets

The plan of the automatic device for replacing the blow-pipe by the diaphragm is shown in the right-hand small figure at bottom

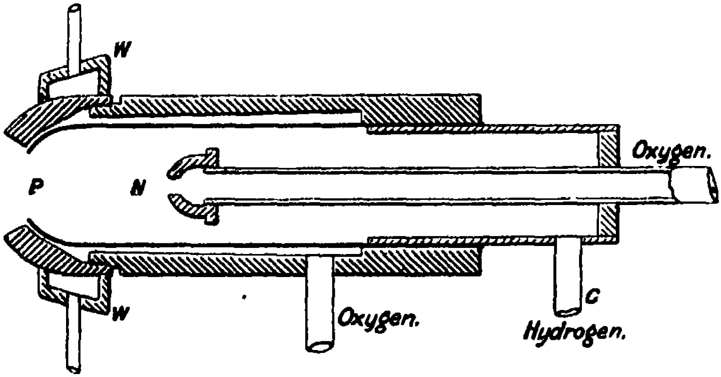
N.B.—This figure is a purely diagrammatic representation of the apparatus, and is not drawn to scale



rubber tubes The gauge R indicates the difference of pressure between the two sides of this diaphragm. By the aid of this gauge R the rate of flow of the gas can be kept constant at any desired value. The gauge G serves to regulate the ratio of the two gases Two bells float in mercury, the interior of one being placed in communication with the oxygen inlet of the blow-pipe, the interior of the other with the hydrogen inlet The apparatus is merely a balance, the two quantities weighed being the pressures of the two gases The point of support is empirically adjusted so that the system is in equilibrium when the gases are flowing in the desired ratio, viz, four volumes of hydrogen to three of oxygen

The instrument is very easy to construct, and its accuracy is amply sufficient for the purpose in view.

FIG 7 —Blow-pipe. (Scale ¾ full size)



The blow-pipe is of a somewhat peculiar shape and is shown in fig 7 About half the oxygen passes through the central nozzle N, which is surrounded by a platinum nozzle P The supply of hydrogen is led in at C, and flows down between these two tubes. The entire arrangement is surrounded by a gun-metal case, which is cooled by the water circulation W. Half the oxygen flows down between the gun-metal and platinum tubes, entirely surrounding the hydrogen where it issues from the platinum nozzle, thus insuring its perfect combustion

The Furnace (F, fig. 6)—The outer shell consists of an ordinary Fletcher furnace, 23 cm. in external diameter. The interior is filled up to within 7 cm. of the top with sand, on which rests one of the ordinary magnesia bricks used for smelting platinum. Their chemical composition is—

Magnesia .	92	per cent.
Lime .	4.3	,,
Silica	2·4	,,
Iron and alumina .	1·3	,,

These bricks are eminently suitable to resist high temperatures. Owing to the presence of silica they could not be used alone for the purpose we have in view, but they are very valuable as an outer crucible in which to place the pure lime on which the platinum rests. In a brick 11 × 11 × 7 cm. a cylinder of 7 cm. in diameter and 3 cm. in depth is drilled out, the hollow is filled up with chemically pure lime in the form of powder, which is pressed tightly in. The platinum, roughly hammered into shape, is forced into the bed thus prepared until the lime is flush with the upper surface of the metal. Before the temperature has reached the melting point of platinum the lime has formed itself into a sufficiently coherent mass to support the weight of the metal. Another brick 11 × 11 × 4 cm. forms the cover; it is held in a cast-iron frame by six ¼-inch screws equally spaced along two adjacent sides of the square. A hole 1·6 cm. in diameter is drilled through the centre of this cover. It is through this hole that the blow-pipe plays vertically on the surface of the metal. The lower surface of the cover should be not more than 1 cm. above the platinum. Both blow-pipe and diaphragm are attached to the same vertical axis A; the electromagnet M, which controls the motion of this axis, is placed in series with the magnet N, which cuts off the supply of gas to the blow-pipe. The instant the gas is stopped the axis swings round, bringing the 1 square centimetre diaphragm D above the hole in the cover of the furnace. A number of screens are provided so as to prevent any light from the furnace itself reaching the photometer. These screens have been removed in fig. 6 so as to give a better view of the furnace.

The light from the platinum is reflected on to the photometer by a mirror. The secondary standard is not placed in the axis of the photometer, but at right angles to it, its light being also reflected by a mirror. Each mirror is supported by a bar, which is held in a socket provided with a V-shaped check, so that when one of the mirrors is taken out of its supporting socket it can always be replaced in the same position; the fittings are made interchangeable. Thus by interchanging the two mirrors any error due to their coefficient of absorption can be eliminated.

A metronome ringing every ten seconds gives the intervals at which the photometric observations are to be made To save time the readings are not taken, but the position of the index on the photometer bar is marked off. The distances are read at leisure later on.

The method by which the observations are obtained is as follows:— Let us suppose that the platinum is in position, the standard of reference adjusted, the metronome started, the "rate" and "ratio" gauges calibrated, and that we are ready to start the blow-pipe. We turn on the gas and increase the supply until the gauge shows that the hydrogen is burning at the rate of 0·8 cubic foot per minute, we then regulate the supply of gas so as to keep the ratio gauge in balance. At the end of fifteen minutes we press the key which stops the gas and swings the diaphragm into position. No photometric readings are taken at the end of this first fusion for two reasons 1. The walls of the furnace have not yet had time to heat up, and the platinum, though fused on the surface, is probably still solid underneath 2 Some particles of lime dust are pretty sure to be floating on the surface if the ingot of metal has only just been put into a newly made crucible. We therefore merely use this fusion to adjust the position of the furnace and cover, so that the centre of the platinum surface and the centre of the aperture in the cover should be on a vertical line passing through the centre of the diaphragm

This done, we re-start the blow-pipe, and fifteen minutes later, as the metronome rings, we press the electric key, keeping an eye on the photometer. The photometer is kept in balance, and every ten seconds, as the metronome rings, the position of the photometer head is marked off By the time some ten or fifteen readings have been recorded the platinum has cooled below its melting point The distances are then read off at leisure, the blow-pipe is swung back into position, the chopper re-set, the mirrors are interchanged, and everything is ready for a fresh start. Fifteen minutes later another series of readings can be taken

A Lummer and Brodhun photometer* and photometer bench were used during these experiments, the distance between the lights being 331·4 cm. The position of both the sources of light was fixed, the photometer head alone being movable. To ensure the maximum sensitiveness it is well to keep one eye exclusively for the photometric observations, covering it when the readings are not being taken with a black screen In all the experiments incandescent lamps were used as standards of reference. When the necessary precautions are taken these form very reliable standards, remaining absolutely constant for many hours.† The pressure on the terminals of these lamps must be adjusted with the greatest care, as the light varies with the sixth power of the electromotive force In the present case fifty-volt lamps were used, a specially constructed divided resistance being placed across the terminals of the lamp By means of a potentiometer the electromotive force on 1/20th of this resistance was compared with the electromotive force of a Clark's cell. To avoid any rapid changes of temperature the cell was placed in the inner chamber of a calorimeter. Under these circumstances the pressure on the terminals of the lamp could be kept constant during many hours to within one hundredth of a per cent.

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* The photometer head and part of the photometer bench are shown in fig 4 To avoid any stray light, the photometer was hung with black velvet curtains. These have been drawn aside in fig. 4. For a full description of this instrument see 'Zeitschrift fur Instrumentenkunde,' p. 41, 1892, and 'J. fur Gasbeleuchtung und Wasserversorgung,' vol 37, p. 61, 1894.

† 'Zeitschrift für Instrumentenkunde,' vol. 10, p 119, 1890

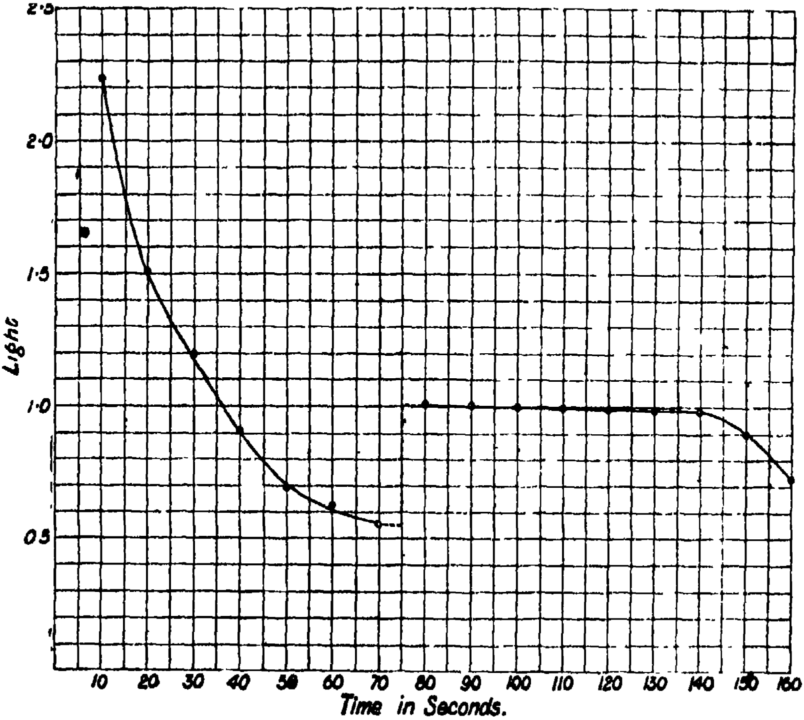
Table V. (See fig 8.)—Normal Conditions.

Intensity of the Light from 1 sq. cm. of Platinum at the Temperature of Solidification as deduced from the observations given below is 1·002 (the error of this determination is therefore 0·002).

Time	Photometer reading.	Light.
10	118·3	2·236
20	133·8	1·503
30	143·0	1·196
40	154·3	0·908
50	164·5	0·693
60	171·8	0·623
70	174·4	0·559
80	150·0	1·008
90	150·3	1·001
100	150·6	0·993
110	150·7	0·991
120	150·8	0·988
130	150·9	0·986
140	151·0	0·984
150	154·8	0·897
160	163·8	0·722

Having in the manner indicated above obtained a set of photometric readings at intervals of ten seconds from the time the blow-pipe was stopped, let us consider what is the best manner of reducing these observations. The typical shape of the curve obtained is shown in fig. 8 (see Table V), the abscissæ represent time, the ordinates light. The curve consists of three parts, the first falling rapidly represents the decrease of light sent out from the liquid metal as its temperature falls. There is a sudden break in the curve when the platinum begins to solidify, followed by a practically straight line. This second period (forty to fifty seconds in the conditions under which these experiments were made) marks the time during which the platinum ingot is freezing. For the sake of brevity we shall in future refer to this part of the curve as the "constant" period, though strictly speaking, owing to the imperfect conduction of the metal, the intensity of the light decreases slightly as the time increases. The third part of the curve represents the cooling of the surface after the entire mass is solid. The want of sharpness in the transition between the second and third parts

FIG. 8—"Normal" conditions total mass of platinum, 345 grammes, superficial area, 17 sq cm.; diameter of aperture in the cover of the furnace, 1 6 cm., intensity of the light at the temperature of solidification, 1·002 (the error of this determination is therefore 0 002).



is due to the fact that the heat lost at the surface is at first supplied by conduction from the lower layers still substantially at the temperature of solidification, and it is not until the corresponding temperature gradient is established throughout the entire mass that the normal rate of cooling is shown by the light given off from the surface.

Curves similar to the above, but referring to the case of molten silver, were shown by Violle in 1881.*

A second form of curve is possible (see figs. 9 and 11) This shape is obtained when the metal has been heated very slightly above its

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* 'Annales de Chimie et de Physique,' sér. 6, vol 3, p. 373, 1884, also Conférence Internationale pour la Détermination des Unités Électriques, séance de la Troisième Commission, 3 Avril, 1884. There is, however, one essential difference between the present results and those obtained by Violle. In the case both of silver and platinum Violle indicates the "flash" as occurring not before but after the constant period.

Table VI. (See fig. 9.)

Superficial area of Platinum Ingot 30 sq. cm.

Intensity of the Light from 1 sq. cm. of Platinum at the Temperature of Solidification – 1·012.

Time.[1]	Photometer reading.[2]	Light[3]
10	145·7	1·119
20	148·2	1·054
30	150·1	1·005
40	149·8	1·013
50	150·1	1·005
60	150·3	1·001
70	151·5	0·972
80	151·7	0·967
90	163·3	0·730
100	178·8	0·502

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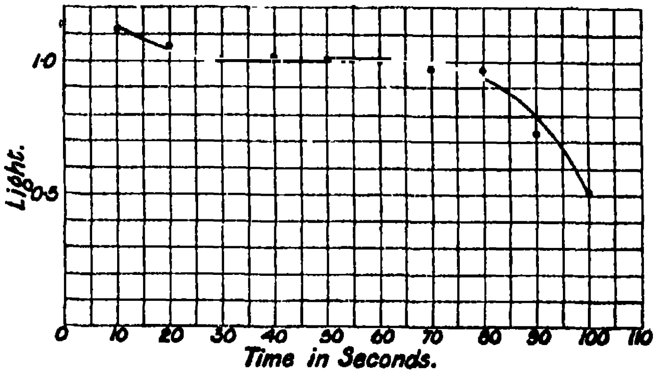
[1] The time is counted from the instant at which the blow-pipe was stopped.

[2] The photometer reading is the distance in centimetres between the photometer head and the incandescent lamp which served as a standard of reference

[3] The unit of light is the mean of a number of determinations made under the "normal" conditions.

Fig. 9.—Superficial area of platinum ingot, 30 sq. cm. (or 76 per cent. above the normal), intensity of the light at the temperature of solidification, 1·012 (or 0·012 above the normal)



melting point. In this case there is no flash up of the light, the three parts of the curve being continuous. The photometric readings, reduced as shown below, vary but little from those obtained when the curves are discontinuous, but for several reasons the results are likely to be less consistent than when the conditions are so regulated as to obtain the "flash" shown in fig. 8

The definition of the standard quantity of light is that emitted in a normal direction by 1 square centimetre of surface of platinum at its temperature of solidification

It has already been pointed out

1. That it is practically impossible to obtain an instantaneous photometric observation
2. That each part of the surface is, strictly speaking, only at the standard temperature for the small time-interval during which the solid film is actually forming
3. That owing to the difficulties inherent to all photometric observations, each determination should rest on a number of separate readings

The most obvious method would be to take the mean of the readings obtained during the "constant period," but in doing so we should certainly be wrong, as these readings form a decreasing series, the first one only being theoretically correct. The rate of decrease of the readings varies with the mass of metal used, and the circumstances under which it is allowed to cool, and were we to take the mean of the readings, the final determination would necessarily be dependent on these experimental conditions The first reading, though theoretically the most correct, is practically the least reliable of the series This reading immediately follows a sudden and considerable change in the illumination of the photometer, and the eye has not yet had time to become accustomed to the change It is therefore out of the question to base our determinations on this first reading

All things considered, it is best to establish the value of the intensity of the light emitted by the platinum at the *instant of solidification* by a graphical exterpolation

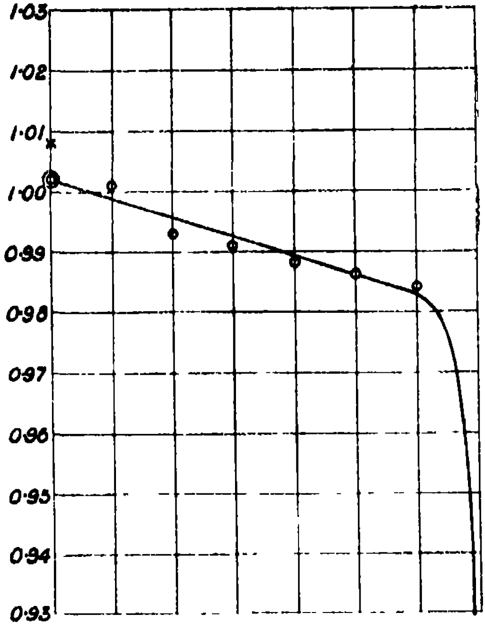
The shape of the curve during the period of solidification is, practically, a straight line. The various observations during this period differ but little in absolute value, and can easily be plotted to a very large scale If we draw a straight line through these points the height of this line above the axis at the time at which the "flash" occurred will give us the quantity of light emitted at the temperature of solidification The values of the observations themselves determine within ten seconds the time at which the "flash" occurred, and for a first approximation this is sufficient.

The above method of reducing the results is illustrated in fig. 10 After a set of observations has been taken, and while the platinum is still white-hot, the surface visible through the diaphragm should be inspected Should any specks of lime dust or other impurity be apparent, the observations should be rejected.

It is advisable to make it a rule to discard any set of readings where:

1. The "constant period" extends over less than thirty seconds.

Fig 10 (see Table V, fig. 8, and p. 495).—Illustrating the graphical method used in reducing the observations.



2. The mean rate of change of the intensity of the light during the so-called "constant period" exceeds 0·03 per cent. per second.

In the course of the present investigation, observations were taken for some three hundred curves, but for obvious reasons only a few of the most typical are recorded here.

It must be clearly understood that the present experiments were not undertaken with a view of obtaining any absolute determinations, but purely as a preliminary investigation. The objects in view were:

1. To ascertain the most favourable experimental conditions.
2. To determine the degree of accuracy to be obtained under these conditions.

The results are expressed in terms of the quantity of light emitted by 1 sq. cm. of platinum at its temperature of solidification, and under the following conditions:—

The total mass used was 345 grammes in the shape of a disc. The area of the upper surface of the disc was 17 sq. cm. The diameter of the hole in the cover of the furnace was 1·6 cm. The observations were taken after the blow-pipe had been alight for fifteen minutes, burning 0·8 cubic foot of hydrogen per minute, the two gases being mixed in the ratio of four volumes of hydrogen to three of oxygen. The above conditions will, for the purpose of this investigation, be taken as the "normal conditions" (see Table VI and fig. 8). It must, however, be borne in mind, that for the final experiments it would be advisable to increase the mass of metal to 1000 or 2000 grammes, and this would entail a change in the quantity of gas burnt.*

This said, let us proceed to determine to what extent the intensity of the light is dependent on the experimental conditions.

Change in the shape of the platinum ingot.—By changing the shape of the ingot we shall modify not only its rate of cooling, but also the maximum temperature it will reach under the "normal" supply of gas. If we flatten out the disc beyond a certain limit, only the central portion of the platinum will actually fuse. On the other hand, if we make the superficial area too small, forming the metal into a long cylinder, a limit will be reached when the fusion will cease to extend to the lower layers. In the present case 35 sq. cm. and 8 sq. cm. are

Table VII. (See fig. 11.)

Superficial area of Platinum Ingot 10 sq. cm.

Intensity of the Light from 1 sq. cm. of Platinum at the Temperature of Solidification = 1·004.

Time[1]	Photometer reading[2]	Light.[3]
10	141·8	1·232
20	144·8	1·144
30	148·8	1·038
40	148·3	1·050
50	149·6	1·018
60	150·3	1·001
70	150·0	1·008
80	150·1	1·005
90	150·2	1·003
100	150·2	1·003
110	151·5	0·972
120	152·8	0·942
130	154·0	0·915
140	154·2	0·910
150	164·8	0·704
160	178·3	0·573

[1] The time is counted from the instant at which the blow-pipe was stopped.

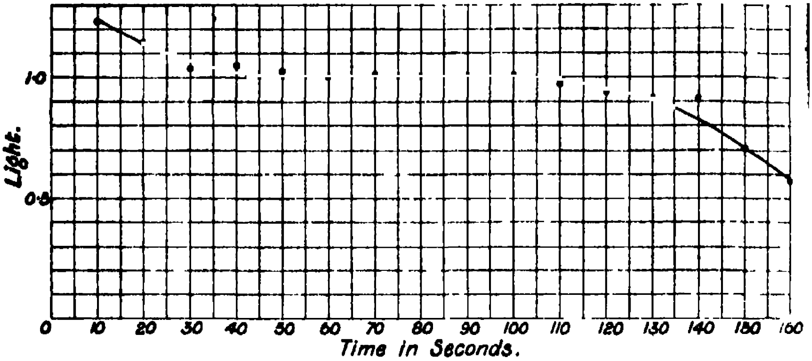
[2] The photometer reading is the distance in centimetres between the photometer head and the incandescent lamp which served as a standard of reference.

[3] The unit of light is the mean of a number of determinations made under the "normal" conditions.

---

* It would also be advisable in future experiments to shape the platinum ingot into a half sphere, as this shape would be preferable with regard to the constancy of the observations during the time the metal is freezing.

FIG 11.—Superficial area of the platinum ingot, 10 sq cm. (or 41 per cent below the normal), intensity of the light at the temperature of solidification, 1·004 (or 0 004 above the normal)



the limiting values. A number of determinations were made with a superficial area of 30 and 10 sq. cm The details of two of the determinations are given in Tables VI and VII, the form of the curves is shown in figs 9 and 11

The maximum temperature reached by the metal was not sufficient to cause the sharp break in the curve of light, but notwithstanding this fact, the values obtained differ less than 1½ per cent. from the "normal" value We are therefore justified in concluding that the *intensity of the light is independent of the shape of the mass*, so long as the entire mass is raised above its melting point

Variation of the mass of metal used—The effect of increasing the quantity of platinum from 345 to 510 grammes is shown in Table VIII and fig 12 In this case the change produced in the light works out at 0 4 per cent. It is not possible to decrease the mass of metal much below 345 grammes, as the "constant period" becomes too short for reliable readings to be obtained For 90 grammes, for instance (see fig. 13), the "constant period" has altogether disappeared, or, strictly speaking, it is only represented by a slight inflection in the curve. The experiments are, however, sufficient to show that though the mass of the ingot has a considerable effect on the degree of accuracy obtainable, it does not affect the quantity of light emitted at the temperature of solidification.

Variation in the shape of the enclosure—From the laws of thermal radiation we are led to expect that the shape and temperature of the enclosure will have a considerable influence on the quantity of light emitted by such a body as platinum. Any change in size of the aperture in the cover of the furnace will greatly modify the rate of cooling of the metal, and from this cause also might affect the photo-

Table VIII (See fig 12)

Mass of Platinum Ingot 510 grammes.

Intensity of the Light from 1 sq. cm of Platinum at the Temperature of Solidification – 1·004

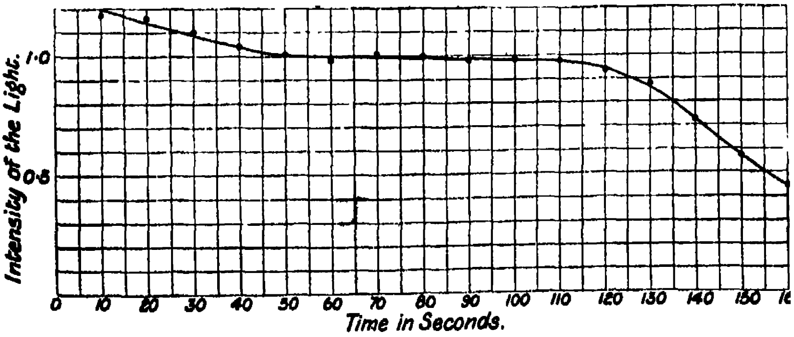
Time.[1]	Photometer reading[2]	Light[3]
10	143·8	1·173
20	144·6	1·150
30	146·5	1·098
40	148·8	1·038
50	150·1	1·005
60	150·8	0·988
70	150·3	1·001
80	150·1	0·998
90	150·9	0·986
100	151·0	0·984
110	151·2	0·979
120	152·8	0·912
130	155·8	0·875
140	163·8	0·722
150	173·8	0·567
160	183·8	0·444

[1] The time is counted from the instant at which the blow-pipe was stopped

[2] The photometer reading is the distance in centimetres between the photometer head and the incandescent lamp which served as a standard of reference.

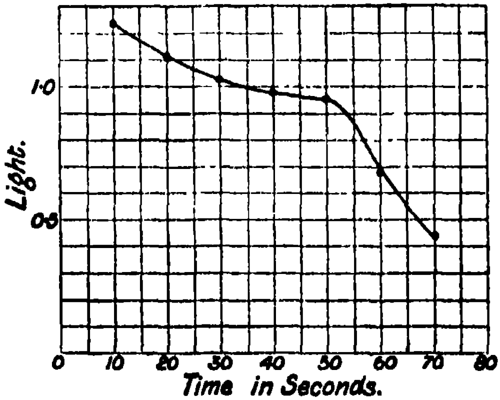
[3] The unit of light is the mean of a number of determinations made under the "normal" conditions.

Fig. 12.—Mass of the platinum ingot, 510 grammes (or 45 per cent. above the normal); intensity of the light at the temperature of solidification, 1·004 (or 0·004 above the normal)



metric observations. The "normal" conditions are chosen in order to make the rate of cooling as slow as possible, and any variation in the circumstances must necessarily cause the platinum to cool more rapidly.

Fig. 13.—Showing that when a small mass of platinum (90 grammes) is used, the light does not remain constant for any appreciable time.



One of the determinations made with the aperture in the cover 2·5 cm. in diameter, or nearly three times the area of the "normal" opening, is illustrated in fig. 14 (see Table IX). The quantity of light differs from the "normal" by less than 1 per cent. If the aperture in the cover is increased much beyond this limit, or if the cover is removed

Table IX. (See fig. 14.)

With the Diameter of the Opening in the Cover of the Furnace increased to 2·5 cm.

Intensity of the Light from 1 sq. cm. of the Platinum at the Temperature of Solidification = 1·005.

Time.[1]	Photometer reading.[2]	Light.[3]
10	147·8	1·064
20	150·0	1·008
30	150·7	0·991
40	150·9	0·986
50	151·3	0·976
60	152·0	0·960
70	153·6	0·924
80	155·8	0·875
90	160·8	0·776

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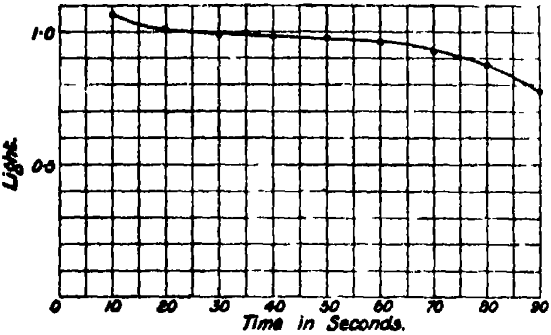
[1] The time is counted from the instant at which the blow-pipe was stopped.

[2] The photometer reading is the distance in centimetres between the photometer head and the incandescent lamp which served as a standard of reference.

[3] The unit of light is the mean of a number of determinations made under the "normal" conditions.

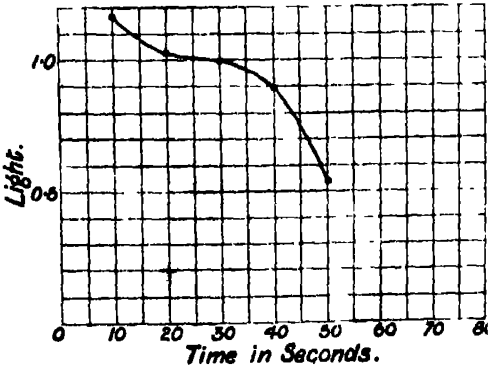
Fig. 14.—Area of the aperture in the cover, 4·9 sq. cm. (or 15 per cent above the normal); intensity of the light at the temperature of solidification, 1·005 (or 0·005 above the normal).



altogether from the furnace, the constant part of the curve vanishes and determinations become impossible. This case is shown in fig. 15.

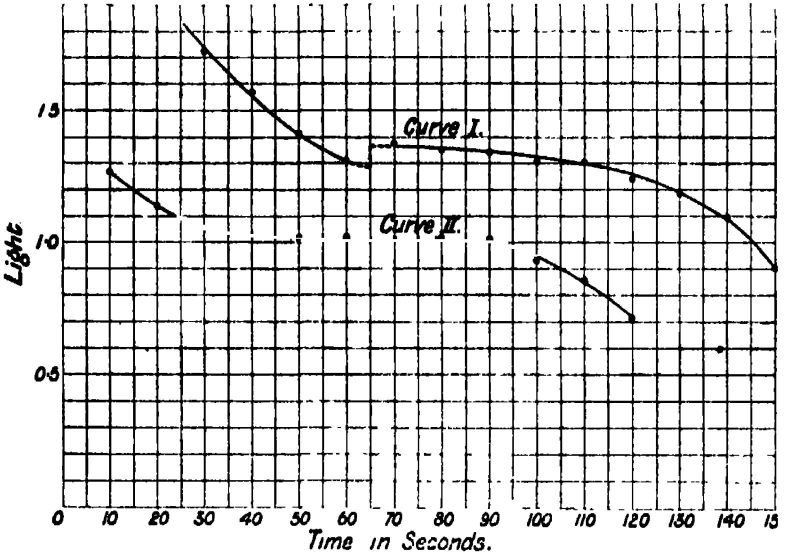
The recommendations made with regard to the rate at which the gases are to be burnt, and to the time during which the blow-pipe is to be alight, are only intended as an indication of the conditions under which the observations will be most easily obtained. A variation of 10 or even 20 per cent. in these factors would leave the final results practically unaffected, but a number of the observations would probably have to be discarded according to one or other of the three rules given on page 495.

Fig. 15.—Showing the effect of removing the cover from the furnace.



The effect of contaminating the platinum with either silica or carbon is very marked. Carbon forms the best illustration, as there is no difficulty in subsequently getting rid of this impurity. On the 26th of March, after a certain number of normal determinations had been made, it was decided to try the effect of using coal gas instead of hydrogen. After the blow-pipe had been supplied for twenty minutes with coal gas, the curve shown in fig 16, curve I, was obtained; fifteen minutes later a similar set of readings were recorded. The entire surface of the metal was covered with a white film. The intensity of the light was 36 per cent. above the normal The coal gas was then stopped, and the hydrogen turned on, the surface gradually clearing as the carbon oxidised out. In an hour and a half the curve fig. 16, curve II, was obtained, the error having already been reduced to less than 2 per cent.

FIG 16.—Curve I shows the effect of using coal gas to heat the platinum, the light is 36 per cent above the normal. Curve II was taken an hour and a half later, the coal gas having been replaced by pure hydrogen The error is already reduced to 2 per cent



From the above results I believe I am justified in stating *that the probable variation in the light emitted by molten platinum under the standard conditions is not above 1 per cent.*

With more perfect apparatus, and with the experimental conditions altered in the direction that has been suggested, the accuracy of this standard would certainly be increased.

Physiological considerations fix a limit to the accuracy of photometric observations. It is not impossible that the accuracy of the platinum standard may attain to or even surpass this limit.

The present work has occupied nearly three years, and it was thought advisable before devoting more time to the subject to publish such results as had already been obtained It is hoped that those who are interested in photometric work will, by their advice and criticism, help to bring the research to a satisfactory close

In concluding, may I be allowed to express my gratitude to the managers of the Royal Institution for placing the splendid resources of the Davy-Faraday Laboratory at my disposal, as well as for generously defraying the necessarily heavy expenses incurred by the present work.

My thanks are also due to Messrs. Johnson, Matthey & Co , who most kindly lent the platinum employed

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* The entry of admission on p. 272 is erroneous.

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